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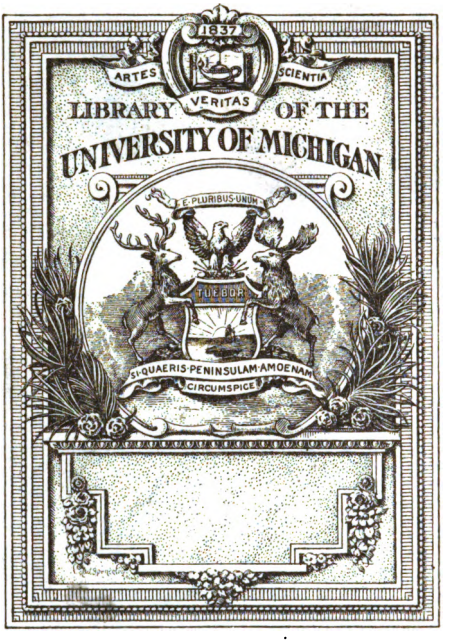

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY,

CONTAINING

PAPERS,

ABSTRACTS OF PAPERS,

AND

REPORTS OF THE PROCEEDINGS

OF

THE SOCIETY,

FROM NOVEMBER 1868, *TO JUNE* 1869.

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VOL. XXIX.

BEING THE ANNUAL HALF-VOLUME OF THE MEMOIRS AND PROCEEDINGS OF THE ROYAL ASTRONOMICAL SOCIETY.

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1869.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIX. *November* 13, 1868.—9 · No. 1.

Admiral MANNERS, President, in the Chair.

Sir William Thomson, Glasgow University;
Rev. E. Crofton, Oak Lawn, Wootton Bridge, Isle of Wight;

were balloted for and duly elected Fellows of the Society.

The Society have received from the Hydrographic Department of the Admiralty Observations of the Total Eclipse of 18th August, 1868, made by Staff-Commander Reed, commanding H. M. Surveying Vessel *Rifleman*, and Mr. Pope Hennessey, Governor of Labuan, at Barram Point, on the N.E. Coast of Borneo. Mr. Pope Hennessey's Observations have been published in the Proceedings of the Royal Society.

The Society have also received from the Peninsular and Oriental Company accounts of the Eclipse, as seen by their Commanders — Captain H. W. King, Latitude 16° 44′ N., Longitude 83° 55′ E., within the totality — Captain C. G. Perrins, commanding S. S. *Carnatic*, Latitude 16° N., Longitude 54° 15′ E., 20 miles north of the limit of totality on that meridian — Captain D. Reynoldson, commanding S. S. *Rangoon*, Latitude 15° 42′ N., Longitude 59° 15′ E., on the central line — and Captain J. S. Murray, then in the Formosa Channel, considerably north of the limit of totality.

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Observations of the Solar Eclipse, August 18th, 1868.

By J. Tebbutt, Esq.

The solar eclipse of the 18th Aug. was pretty well observed here. Although the Sun was occasionally hidden from view by passing clouds, the beginning and the greatest obscuration were well seen. A remarkable phenomenon presented itself about half a minute before the instant of first contact. A dark streak or band became visible for several degrees along that limb of the Sun to which the Moon was approaching, and after continuing visible for a few seconds, vanished. It reappeared, however, for a moment immediately before the contact. The darkness seemed to extend along the narrow interval separating the limbs of the Sun and Moon, but did not encroach on the solar disk. This phenomenon was so palpable that it served as an excellent warning of the time of contact. A very slight flattening of the Sun's limb was also remarked just previous to the contact, but this was quite distinct from the indentation produced by the Moon. The Sun was shining beautifully clear at the time. The first contact was pretty accurately observed at $4^h\ 6^m\ 30^s$, local mean time, the telescope employed being my $3\frac{1}{4}$-in. refractor with a power of about 30. During the progress of the eclipse there was a slight boiling of the limb both of the Sun and Moon. This, however, was the result of atmospheric causes, and was not so great as I expected from the low altitude of the objects. With the exception of the slight disturbances from this cause, the Sun's cusps were sharp and well defined. A careful scrutiny satisfied me that not the slightest portion of the Moon's limb could be even faintly distinguished beyond the cusps. No remarkable irregularities were observed on the Moon's limb. Two very fine spots, each surrounded with an extensive penumbra, and attended with several smaller spots, were very conspicuous on the southern half of the Sun's disk, but were not eclipsed by the Moon. A magnificent tract of faculæ, situated near the larger spot, contrasted very beautifully in colour with the general ground of the solar disk. About one-third of the Sun's diameter was eclipsed at the time of greatest obscuration. The Sun disappeared behind a dense cloud on the horizon at $5^h\ 16^m$ P.M.

Meteorological observations were taken through the day, but nothing remarkable, beyond the unusual steadiness of the barometer, was recorded. The observations of the black-bulb thermometers were much interfered with by passing clouds.

Windsor, New South Wales,
September 1st, 1868.

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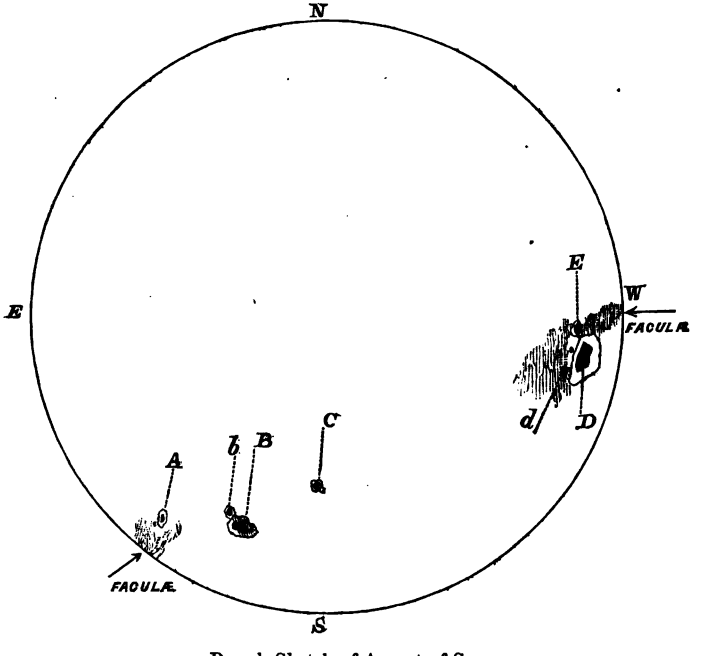
Sun-spots and general Aspect of the Sun on the day of the Total Eclipse, August 18th, 1868.

(*Communicated by Messrs. De la Rue, Stewart, and Loewy.*)

The history of the great eclipse would, in our opinion, be incomplete without a record of the heliographical positions of the Sun-spots observed at the time.

The unfavourable weather in England on the days before and after the eclipse prevented photographic observations, but we were so fortunate as to obtain a sun-picture on the day of the eclipse, at $3^h\ 29^m$ G.M.T., about eight hours after the phenomenon.

There were two comparatively large groups of spots on the Sun, one near the eastern, the other near the western, limb. Both groups were connected, as shown in the accompanying rough



Rough Sketch of Aspect of Sun.

sketch, with faculous matter which apparently extended beyond the visible disk on both sides. The faculæ consisted chiefly of very numerous small scattered patches, here and there running together and forming larger flocculi; they covered altogether an unusually large area, indicating the extraordinary activity which

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was also observed by Dr. Janssen, during and after the time of the eclipse, on the surface of the Sun.

The angles of position of those two parts, where the faculæ seemed to extend into the invisible hemisphere, may roughly be stated as 145° for one patch, and 255° for the other, reckoning from north towards east. How far these positions agree with any observed protuberances can only be gathered after some time, when the whole of the detailed accounts have come to hand.

The heliographical elements of the Sun-spots, forming the two groups, were calculated in accordance with the principles explained in a paper read before the Royal Society some time ago, which we hope to see soon in the hands of the printer. These elements are given in the following table, where the letters for each spot correspond with those on the accompanying sketch.

Heliographical Elements of the Sun-spots, observed at the Kew Observatory, on August 18, 1868, $3^h\ 29^m$ *G.M.T.*

Spots.	Scale-Reading.	Dist. from Centre.	Dist. in terms of Radius.	Circle-Reading.	Angle of Position.	Heliographical Lat.	Heliographical Long.
				° ′	° ′	° ′	° ′
A	0·287	1·729	0·908	135 18·0	135 3·8	−21 34	191 51·2
B	0·564	1·452	0·763	147 3·2	146 49·0	−23 37	211 40·0
b	0·563	1·453	0·763	145 22·5	145 8·3	−22 34	210 52·7
C	0·810	1·206	0·633	158 38·3	158 24·1	−23 10	225 57·3
D	0·271	1·745	0·917	251 52·6	251 38·4	−28 55	309 30·8
d	0·317	1·699	0·892	250 23·3	250 9·1	−28 47	305 29·1
E	0·303	1·713	0·900	254 3·1	253 48·9	−26 12	307 59·4

On a possible Method of viewing the Red Flames without an Eclipse. By William Huggins, F.R.S.

In the Report of my Observatory at the last Anniversary (p. 88 of the last volume), it is stated that "during the last two years numerous observations have been made for the purpose of obtaining a view of the red prominences seen during a solar eclipse. If these bodies are gaseous their spectra would consist of bright lines. With a powerful spectroscope the light reflected from our atmosphere near the Sun's edge would be greatly reduced in intensity by the dispersion of the prisms, while the bright lines of the prominences, if such be present, would remain but little diminished in brilliancy. This principle has been carried out by various forms of prismatic apparatus, and also by other contrivances, but hitherto without success." The observations of the eclipse of August last having shown the position in the spectrum of the bright lines of the red flames, Mr. Lockyer and Mr. Janssen succeeded independently, by a similar method, in viewing the spectra of these objects.

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My object in this Note is to describe one of the "other contrivances" mentioned in the Report.

The apparatus consisted of screens of coloured glasses and other absorptive media, by which I was able to isolate portions of the spectrum. It appeared highly probable that if the parts of the spectrum which then alone remained were identical with those in which the bright lines of the flames occur, these objects would become visible.

For this inquiry I obtained a great variety of coloured glasses and other absorptive media. I first examined them with a prism to learn the absorptive power which they exercised on different parts of the spectrum. I then combined them in various ways. These glasses were sometimes employed before the eye, but more frequently by projecting the image of the Sun's edge upon a screen, after the light had been sifted by the coloured media. In making these experiments means were taken that the whole of the Sun's image should be got rid of, in order that the eye, kept in comparative darkness, might be more sensitive to the greatly feebler illumination of the objects sought for. As I had no knowledge of the position in the spectrum of the bright lines, it would have been by accident only if I had succeeded in obtaining a view of the flames.

Now that the positions of these lines are known, this method appears to be very promising. Perhaps the light about the red line at C will be most easily isolated. I have a deep ruby glass which cuts off all the spectrum except the extreme red. I have since the observations only been able to make one attempt, when the state of the atmosphere was unfavourable.

It is obvious that by this method the form and appearance of these flames could be observed, and the objects measured with accuracy.

On a possible Method of viewing the Red Flames without an Eclipse.

(*Extract from Letter from Sir John Herschel, Bart. to the Rev. C. Pritchard.*)

I send you an extract from a letter of my son, Lieut. Herschel, from which you will see that immediately on his ascertaining the fact that the light of the protuberances exhibited three *bright* lines corresponding to *dark* lines in the light of the Sun's photosphere, he was at once led to speculate on the possibility of taking advantage of this fact to obtain a view of the protuberances apart from the condition of a total eclipse. That such *glasses* as he contemplates (or other absorptive media) could be found, there is, I fear, little hope; and my only view in calling attention to this suggestion is to show how immediately and readily a clearly defined new fact of this sort suggests to an active and combining intellect the possibility of immediate practical application. If I mistake not, the "double discovery" of Mr. Lockyer and Mr. Janssen, of a mode of rendering the pro-

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tuberances *sensible* (not visible) by spectroscopic analysis of the light close to, *but beyond* the photosphere in ordinary daylight, turns upon the very same feature — viz. the *absence* of the three rays D, &c. in the photospheric light (and therefore in the *general* skylight) and its *presence* in that portion of the skylight which is superposed on the prominences; so that, while skylight *in general* exhibits necessarily three *black* lines corresponding to D, &c. — at that particular spot where a prominence exists, they should not be exhibited at all, or very feebly.

Extract of a Letter from Lieut. J. Herschel, dated Bangalore, Sept. 2, 1868.

"It has occurred to me that, since the whole light of the 'flames' is of three refrangibilities only (or nearly so), dark glasses (could they be formed) which allowed these *only* to pass, would so enormously diminish the light *from the Solar disk*, as to enable the flames to be seen without the interposition of an opaque body, *i.e.* without an eclipse. When I say dark glasses, I mean, of course, *any* media which have the effect required. Is the idea a practical one? If it is, these flames may come to be studied *at leisure!*

"I will not forget *η Argûs* you may depend."

Collingwood, Nov. 3, 1868.

Remarks on Mr. Stone's Rediscussion of the Transit of Venus, 1769. By S. Newcomb, Esq.

I beg leave to offer to the Royal Astronomical Society the following remarks on Mr. Stone's very interesting re-discussion of the observations of the transit of *Venus* in 1769, which appears in the October number of the *Monthly Notices*.

Mr. Stone's interpretation of Chappe's observation of first contact at egress seems to me untenable. To understand the question at issue it must be borne in mind that, owing to irradiation, after the planet has apparently entered upon the solar disk, it is still connected with the latter by a dark protuberance, or "black drop," which rapidly grows thinner, and at last breaks off, leaving the planet at a sensible distance from the edge of the disk. This phenomenon marks the true time of external contact, and is that to which the attention of the observer should always be directed. At the second external contact the same phenomenon may be observed in a reverse order, a black ligament suddenly forming between the planet and the edge of the solar disk.

The question is whether, at this contact, Chappe observed the sudden formation of the ligament, or whether he waited for the moment when the apparent limbs of the two bodies would have touched each other if the drop had not been there. Chappe says (I quote from Mr. Stone):

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"Temps vrai $5^h\ 54^m\ 50^s$. Premier contact à la sortie. * * * A ce premier contact *Vénus* s'est allongée plus considérablement que le matin en s'approchant tout-à-coup du bord du Soleil."

If, as Mr. Stone assumes, the time given in the first sentence of this extract does not refer to the phenomenon described in the last sentence, it must be either (1) that by the words "ce premier contact" Chappe meant something different from the "premier contact" he had observed, or that by the preposition "à" he meant some time before. His language does not seem to me to admit of either of these meanings. Moreover, from his account of the ingress it appears that Chappe was acquainted with the phenomena, and knew what was to be observed. "Je n'observai point pour l'instant de l'entrée total celui ou le bord de *Vénus* commençoit à s'allonger, mais ne pouvant pas douter que ce point noir ne fît partie du corps opaque de *Vénus*, j'observai le moment où il étoit à sa fin."

Mr. Stone's view does not seem at all strengthened by the observation of Pauly, for Chappe says he saw *Venus* perfectly after Pauly had left his telescope, and intimates that the reason of his leaving so soon was his want of practice in observation. But if Mr. Stone's view is right, and if Chappe saw the phenomenon he describes, Chappe must have seen the very thing he wanted to observe at the moment Pauly left the telescope. His last remark would then hardly be consistent with honesty.

A re-solution of Mr. Stone's equations on the hypothesis here maintained leads to the following results:—

Normal Equations.

$$16x - 64{\cdot}4\,\delta\varpi = +358{\cdot}0$$
$$21y - 110{\cdot}2\,\delta\varpi = -56{\cdot}4$$
$$-64{\cdot}4\,x - 110{\cdot}2\,y + 123134\,\delta\varpi = +45536.$$

Solution.

$$x = +24^s{\cdot}0 \qquad y = -0^s{\cdot}6$$
$$\delta\varpi = +0''{\cdot}38 \qquad \varpi = 8''{\cdot}87$$

If the equations in x and y are treated separately, the results will be—

From equations in x, $\varpi = 8''{\cdot}91$
From equations in y, $\varpi = 8''{\cdot}70$.

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A Reply to Mr. Newcomb's Remarks on "Mr. Stone's Rediscussion of the Transit of Venus, 1769." By E. J. Stone, Esq.

I would remark, in the first place, that Mr. Newcomb has not touched any point of primary importance in this discussion. The real issue between the old value, of 8″·58, and mine, of 8″·91, is no less than 0″·33. The point Mr. Newcomb has raised is a question of only 0″·04, viz. between my value and 8″·87. The question is, therefore, one of comparative insignificance, but the observations will not, I believe, bear even this slight change.

Mr. Newcomb's point is, that I have erroneously assumed that Chappe, at the egress, observed an apparent internal contact instead of the real one or formation of the black drop. His only argument is that it would be absurd to suppose that he would do so. I should have thought that my meaning was sufficiently clearly expressed. My words were, "I consider, from the description given by Chappe himself, that at the egress he did not catch the formation of the black drop. He appears to have been surprised by the contact being established all at once." Take these words in conjunction with Chappe's description of his observation, "Le soleil étoit ondoyant ainsi que *Vénus*, ce qui rendoit cette observation très difficile. A ce premier contact *Venus* s'est alongée plus considérablement que le matin, en s'approchant tout-à-coup du bord du Soleil." It appears to me you have exactly my meaning, that although Chappe fully intended to observe the same phenomenon at the egress which he observed at the ingress, yet owing to tremor and the difficulty of the observation he did not catch the first formation of the drop, but some phase much nearer to the apparent internal contact. Calling to mind Wales' observation of 24ˢ between the real and apparent contacts at the egress, I maintain, in spite of Mr. Newcomb's remarks, that Pauly's positive observation of same contact 22ˢ before the time given by Chappe does strengthen most materially the inference which I have drawn from Chappe's own words.

It is an unfortunate fact for Mr. Newcomb's argument about the "premier contact," that at the ingress Chappe describes the phenomenon observed not as a "contact" but as "l'entreé totale." Mr. Newcomb appears to me to have mixed up his own inferences and Chappe's words respecting Pauly and his observation. Pauly appears to have occupied a position equivalent to that of an English Royal Engineer. Chappe says that he was an observer of a little, *i.e.* some experience. An entirely different impression from that given by Mr. Newcomb's words.

Again, when Mr. Newcomb says that "Mr. Stone's view does not seem at all strengthened by the observation of Pauly, for Chappe says he saw *Venus* perfectly after Pauly had left his telescope," Mr. Newcomb has overlooked the fact that this disappearance of *Venus* refers to the *total egress* of the planet from

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the disk, and therefore proves nothing with respect to Pauly's observation of internal contact about 19^m before.

The observations appear to have been arranged as follows: An assistant was stationed near the clock to give out the time, the observers themselves only comparing their time with the clock directly as a check. There is no doubt but that Chappe, with his more powerful instrument and the extraordinary care and precautions he took to catch the last glimpse of the planet on the Sun, did see *Venus* long after Pauly had lost sight of the planet. Chappe distinctly states that without the precautions he adopted to secure this last glimpse an enormous error would have been made. This, however, does not prove that Pauly's positive evidence of a contact, an entirely different and much easier observation, should be entitled to *no weight*. Chappe undoubtedly thought that Pauly's observation was too early, because he had not himself caught it. I consider this accounted for by the difficulty of the observation owing to tremor.

I am quite at a loss to find what evidence Mr. Newcomb has collected that Pauly left the telescope after the internal contact at egress, and that Chappe, his attention being thus called to the point, looked particularly and could then see no trace of the contact being established. There is no such statement in Chappe's journal, and Chappe never lived to return. Mr. Newcomb has blended together Chappe's remarks relating to the total egress, or final disappearance of the planet from the Sun's limb, and applied them by construction to the observation of internal contacts. If Chappe has made no remarks about Pauly leaving the telescope near the time of internal contact, Mr. Newcomb's dilemma has no force. I have, perhaps, dwelt too long upon this point, but it is the only one raised by Mr. Newcomb. There is a difficulty respecting these St. Joseph observations which has apparently escaped Mr. Newcomb's notice. There were three observers near St. Joseph, Chappe, V. Doz, and Medina. The observed durations agree so closely that whatever assumption be made respecting the one must be made respecting all. It certainly appeared to me so arbitrary an assumption that all three observers had missed the first formation of the black drop at the egress that, in spite of Chappe's language and Pauly's observation, I at first assumed that real contacts must have been observed. The value of solar parallax deduced from the equations on these suppositions was perfectly satisfactory to me, but the residual far exceeded any probable errors and the difference in time between the two phases was not satisfactory. I then, on re-consideration, came to the conclusion, which I still think satisfactory, that if Chappe's words indicate that he lost the first appearance of the black drop, the same atmospheric disturbances, the same boiling of the limbs of the Sun and *Venus*, similar difficulties of observation, in fact, might equally prevent the first appearance being caught by V. Doz and Medina, who were observing near. My argument is this,—We have two distinct phases of a pheno-

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menon, the real and apparent contacts, separated roughly by about 20^s: a quantity too large to be confounded with mere error of observation. If an observer states positively that he has observed a real contact, I have no right to assume that he has observed an apparent contact. If the observer says that he has observed an apparent contact, I have no right to assume that he has observed a real contact; but if the observer's words are doubtful, and I find that, by assuming he has observed an apparent contact, all the observations are rendered accordant, whilst by assuming that he has observed a real contact, all the observations are rendered discordant, then I maintain that it is not only permissible, but necessary, that we should assume that apparent contacts were observed.

The appeal in such a case must be to the residual errors. I have here placed my argument on its lowest ground. I believe that Chappe's own words require the assumption which I have made.

The following are the residual errors in my solution and in the modified form proposed by Mr. Newcomb.

Observer.	Stone's *solution* s	Newcomb's *modification*	Mean for S.	Mean for N.
Hell	− 1·7	− 0·8		
Sajnovics	+ 2·8	− 5·3	+ 0·6	− 3·1
Wales	+ 0·4	+ 7·9		
Dymond	− 0·6	+ 8·9	− 0·1	+ 8·4
Rumovsky	− 0·9	+ 6·9	− 0·9	+ 6·9
Chappe	+ 0·5	− 6·6		
V. Doz	− 0·4	− 5·7	− 1·6	− 4·3
Medina	− 5·4	− 0·7		
Green	+ 5·8	− 1·8		
Cook	− 4·2	+ 8·2	+ 0·8	+ 3·2

Mr. Newcomb's modification has increased the sums of the squares of the residuals at the five stations nearly 40 times.

It was these residuals, the unsatisfactory value of $\frac{x-y}{2}$, and Chappe's own language concerning the difficulty of the observation at egress, the establishment of the contact all at once, and Pauly's positive observation of a contact 22^s before Chappe's observation, that drove me to assume that Chappe did not, as a matter of fact, catch the first formation of the drop, but some phase much nearer the apparent contact. Mr. Newcomb has only reproduced one of the many slight variations I have tried upon this question.

The following table will perhaps show how well this transit of *Venus* of 1769 was observed; how favourable, on the whole,

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were the positions of the observers; how impassable a barrier any satisfactory discussion of these ten observed durations places between such values of the solar parallax as 8″·58 and 8″·91.

If the value 8″·58 of the solar parallax satisfied the ten observed durations, we should have to assume the following errors to have been made in the mean of the observed durations at each station, in order that 8″·91 could be admitted as the true value of the solar parallax.

+ means that the observed duration is too small.

Station.	No. of Obs. durs.	Greatest discordance.	Assumed Mean Error.	Stone's Residuals.
		s	s	s
Wardhus	2	4·5	+24·9	+0·6
Kola	1	..	+25·3	−0·9
Hudson's Bay	2	1·0	+ 7·8	−0·1
St. Joseph	3	5·9	−10·3	−1·6
Otaheite	2	10·0	−27·5	+0·8

Such errors, as the results of observation, are simply impossible, and if a satisfactory solution of these ten durations had ever been obtained with a value of the solar parallax 8″·58, then, whatever might have been the results derived from our other methods, the true value of the solar parallax could not have been 8″·91, nor any value near it. The assumed mean errors show most clearly the grounds upon which astronomers refused to give up the value 8″·58. The error was impossible, only provided the discussion of the results had been satisfactory. The solutions obtained, however, with the value 8″·58 have never satisfied the observed durations. Exaggerating the importance of some alterations in Hell's Journal, a solution has been forced by virtually assuming the observations at Wardhus to have been forgeries. Even this, however, has not been sufficient, the Kola observations still stood out as a warning. In my solution I have rejected nothing for discordance. The residuals are all reduced, in the mean results, within 1ˢ·6, a quantity within probable errors of observation. I consider, therefore, that this great problem of the determination of the mean distance of the Sun from the Earth, may be considered as almost settled within the requirements of our present astronomy. All our work is now accordant, and points to a value about ninety-one millions seven hundred thousand miles.

Observations made at the Royal Observatory, Greenwich, of the Egress of the Planet Mercury from the Sun's Disk, 1868, *Nov.* 4.

(*Communicated by the Astronomer Royal.*)

Observations by E. J. Stone.

The instrument employed was the Great Equatoreal. Aperture of object-glass, 12¾ inches.

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My attention was chiefly directed to any peculiarities which might be presented during the transit, and not to time-determinations.

With power 137, a ring of light was clearly visible around the disk of *Mercury*. It extended to a distance of nearly a semi-diameter.

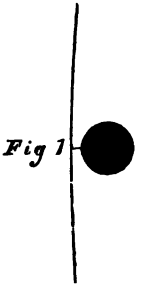
The ring was still visible with a power of 245, and did not disappear with any changes in the coloured glasses employed. I am of opinion that it arose from mere contrast.

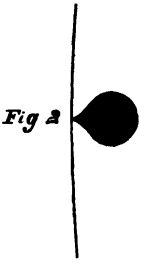
The central part of the disk was certainly blacker than the edges.

I could see no central bright spot.

I could see no satellite.

At the first contact, without the slightest deviation from circular form, a very fine ligament was suddenly seen, connecting the planet and the Sun's apparent limb (fig. 1). This ligament





very quickly became broader, and made it extremely difficult to estimate the time of apparent internal contact, which, however, I estimated at about fifteen seconds after the first phenomenon of the connecting ligament. On looking at the half-second chronometer after the formation of the ligament, the transit-clock time was $11^h\ 59^m\ 14^s{\cdot}5$, but this was some seconds after the first formation of the ligament. The corresponding G.M.T. would be $21^h\ 0^m\ 5^s{\cdot}5$.

Observations by Edwin Dunkin.

The observations were made with the Altazimuth; power about 100; clear aperture of object-glass $3\frac{3}{4}$ inches:

	h	m	s	
First contact	21	0	6·2	G.M.T.
Last contact	22	2	35·7	

The time recorded at the first contact was when the limb of the planet was first seen in coincidence with the Sun's limb, at the moment when it was noticed suddenly to assume a balloon or pear shape. I could not see any very narrow filament between

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Mercury and the Sun's limb before the recorded time. The apparent internal contact of the limbs must have been about 10^s after the recorded time. At the first contact the appearance was similar to that represented in Fig. 2.

Mercury was densely black in the interior parts of the planet, but near the limbs I thought that there were appearances of a slight illumination. The limbs had a hazy appearance. No bright spots seen on *Mercury*. The observed times were registered on the chronograph.

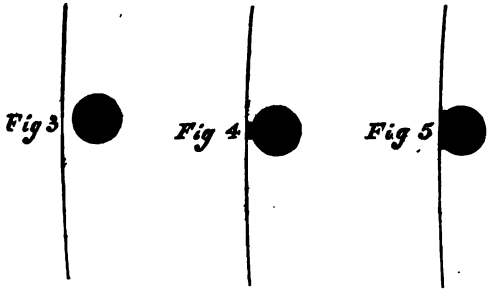
Observations by G. S. Criswick.

The observations were made near the thermometer stand on the magnetic ground, with the 46-inch telescope, and power of 170. The times were observed with a mean solar chronometer (Carter 154) which was compared by means of coincidences with the transit-clock directly afterwards.

	h	m	s	
First contact	21	0	12·8	G.M.T.
Last contact	21	2	42·8	

The limb of *Mercury* seemed very rough when seen on the Sun's disk.

Just before the planet arrived at the Sun's limb, it became somewhat elliptical (fig. 3), and then suddenly sent out a black



point to the Sun's limb (fig. 4): this was the phenomenon observed for "first contact:" the time given is perhaps two or three seconds late, as I was not counting the chronometer at the time. The observation of the last contact is considered pretty accurate.

The distortion of *Mercury* made the time of "bisection" too uncertain to be worth observing.

Observations by W. T. Lynn.

The observations were made with the North Equatoreal, power about 170.

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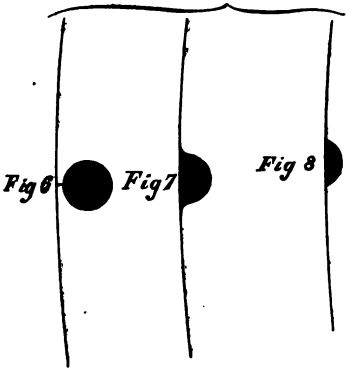
	h	m	s	
First contact of filament	21	0	0·7	G.M.T.
Contact of limbs established by	21	0	32·6	
Bisection	21	1	31·5	
Last contact	21	2	37·3	

The planet as it approached the Sun's limb appeared, as it were, elliptical on the side nearer the Sun's centre.

The first time noted is when a thread or filament of blackness seemed to issue from the planet and touch the Sun's limb (fig. 6). It is considered pretty exact.

The time of contact of limbs is doubtful, the planet assuming the shape shown in fig. 7. The time is, therefore, an estimation that they were certainly in contact by at least that time.



The time of bisection is probably true to within about 2 or 3 seconds (see fig. 8).

The last contact is, I believe, true within a second, as I certainly saw an indication of the planet a second before the time noted, at which time it had totally disappeared.

The Sun's limb was well defined, and the tremor by no means considerable.

Observations by J. Carpenter.

The telescope used was one of the former collimators of the Transit Circle; 3¾-in. aperture, 62-in. focus, power 90.

	h	m	s	
First contact	21	0	14·1	G.M.T.
Last contact	21	2	40·1	

At the first contact the disk of *Mercury* was distorted as shown in fig. 9.

The black ligament appeared to form instantly and to be of the breadth indicated, about ⅓ the diameter of the planet.

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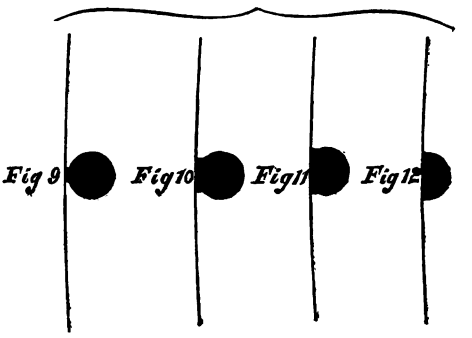
The same character of distortion remained till the planet crossed the edge of the Sun; the successive phases being as shown in figs. 10, 11, 12.



The squareness of outline of the black ligament was very striking; its appearance was not altered by varying the density of the glass shade. A graduated shade was used.

Observations by H. J. Carpenter.

The observations were made with the East Equatoreal; power 70.

	h	m	s	
First contact	12	0	11·1	G.M.T.
Last contact	12	2	36·7	

The time noted for first contact is that of internal contact of the limbs when light first ceased to be seen between them. That of last contact is considered pretty exact.

Some Remarks and Suggestions arising from the Observations of the Transit of Mercury across the Sun's Disk, Nov. 4, 1868. By E. J. Stone, Esq.

The diagram with this paper is intended to represent the phenomena presented at the ingress and egress of *Mercury* and *Venus* across the Sun's disk. I have allowed about $\frac{1}{60}$th part of the diameter of *Venus* for the effects of irradiation. The extreme boundary of light and shade represents the apparent limb of the Sun. The true limb of the Sun may be considered to fall half way between A B, and the boundary of light and shade. When the true limb of the planet reaches the true limb of the Sun, or the apparent limb reaches A B, the light is cut off, and a fine dark ligament appears to connect the limbs of the planet and the

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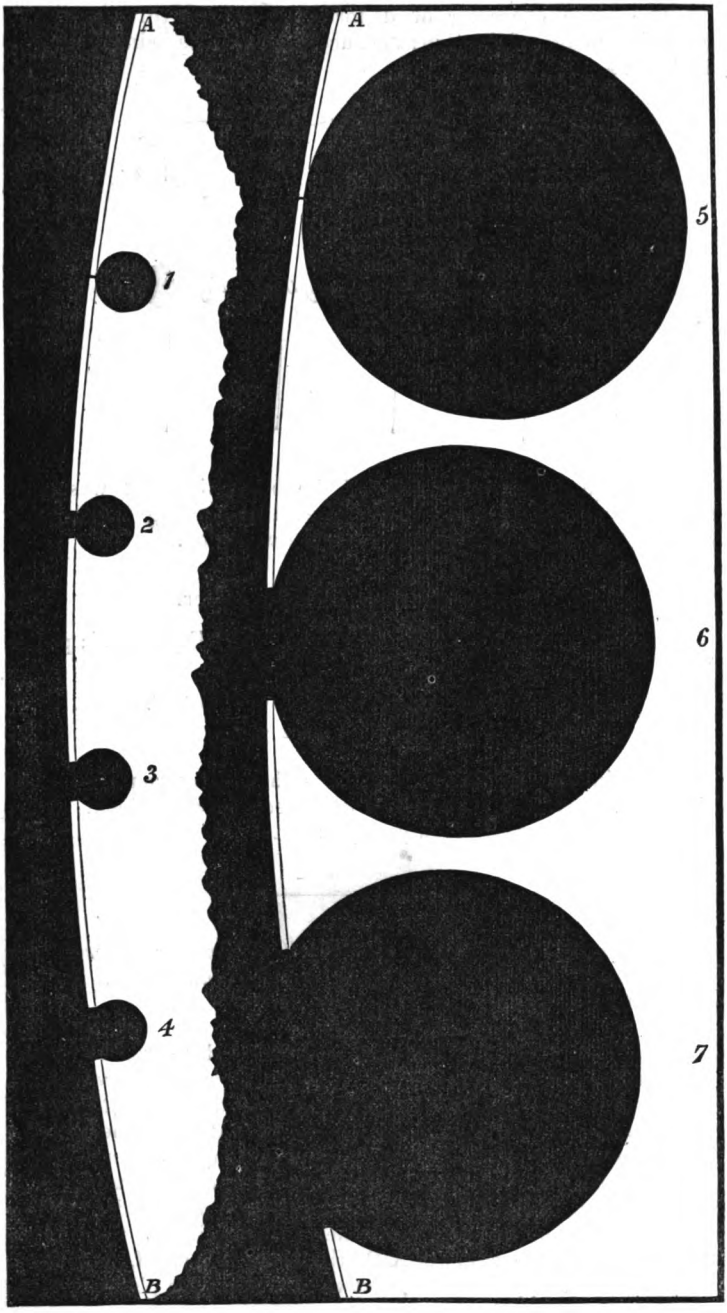

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Sun. This is the true contact: the time of the formation or breaking of the so-called black drop.

Figs. 1 and 5 represent this phase respectively for *Mercury* and *Venus*.

Figs. 3 and 6. The appearances presented at apparent internal contact.

Figs. 2, 4, and 7. Appearances at intermediate phases.

Fig. (7) is intended to represent the phase of figs. 2 and 4 of Cook's and Green's observations in 1769, the point of comparison being the breadth of the ligament.

With respect to the Greenwich observers, the first appearance of the contact appeared to me to take place without the slightest previous distortion of the form of *Mercury*, the ligament appearing almost like a cobweb for fineness.

Mr. Lynn appears to have first caught a phase very near that of fig. (1). Mr. Dunkin, a phase between figs. (2) and (3). Messrs. Criswick, James Carpenter, and H. Carpenter, phases somewhere between (2) and (3).

Mr. H. Carpenter has not given any drawing. He stated, however, that he tried to catch the time when the thread of light entirely disappeared. On my requesting him to point out which of the figures appeared to him to represent most nearly the appearance of the planet at the time of his observation, he, without hesitation, pointed out fig. (3). His time agrees closely with the time given by Messrs. Criswick and James Carpenter. I would particularly call attention to the differences in aperture and optical power of the instruments employed by the different observers.

It appeared to all of the observers difficult to catch the time of apparent internal contact; figs. (3) and (6) are intended to show the comparative facility with which this phase can be seized in the case of *Venus*.

As practical hints for guidance in future observations of a Transit of *Venus*, I would suggest for consideration the following points:

1. That telescopes of nearly the same aperture should be employed.
2. That very nearly the same magnifying powers should be employed by all the observers.

(3.) That attention should be directed to observations of real internal contacts as the chief points.

Phase Fig. (5).

(4.) That apparent internal contacts should also be most carefully observed.

Phase Fig. (6).

5. That in observing apparent internal contacts two times should be given corresponding to the "Contactus

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dubius" and "Contactus certus" of Hell and Sajnovics, and the mean taken for the time apparent contact.

6. That diagrams or drawings should in all cases be furnished by the observers to represent, as nearly as possible, the appearance of the planet corresponding to each recorded time of observation. In making such drawings the essential point should be to represent accurately the comparative breadths of the ligament with respect to the diameter of *Venus*. It must be remembered that such words as "instantaneous" refer only to the observer.

I lay more stress upon the observation of apparent internal contacts, because I believe that under unfavourable atmospheric circumstances, the real internal contacts would not be generally caught by the observers until long after the contact had been established.

Observation of Transit of Mercury, end of, on November 4, 1868. By Professor C. Piazzi Smyth.

Telescope aperture = 6·4 in.; magnifying power = 80.

Definition very bad, from undulations in the atmosphere sometimes so excessive as to cause the planet, otherwise signally black and round, almost to disappear; and making its circumference and the Sun's limb always to consist, more or less, of broken ripplings of light and shade, completely preventing thereby any real phenomenon of the expected "*rupture of the luminous thread*" at internal contact.

	G.M.T. h	m	s
Internal contact, thus unfavourably observed, at	21	0	7
External contact, or planet completely off the Sun's disk, at ..	21	2	20

Royal Observatory, Edinburgh,
November 12, 1868.

The Transit of Mercury, November 4, 1868, observed at the Observatory, Durham. By J. J. Plummer.

(*Communicated by Prof. Chevallier.*)

The transit of *Mercury* was observed at Durham under exceptionally favourable circumstances; the Sun rose in a perfectly cloudless sky, and not a cloud was seen during the whole course of the observations. The limb of the Sun was, however, very

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tremulous, and the planet ill defined, owing to the low altitude; but both improved very much towards the time of egress.

The instrument used was the Fraunhofer Equatoreal of the Observatory. Aperture, 6½ inches, and 8 feet 3 inches focal length, furnished with a wire-micrometer; power 112.

The times of the internal and external contacts at the egress were observed as follows:—

	Durham S.T.	Green. M.T.
	h m s	h m s
Internal Contact	11 53 21·0	21 0 12·0
External Contact	11 55 38·5	21 2 29·2

The first time is considered quite exact; the second, owing to the undulations of the Sun's limb, is less certain.

Some measures were made of the differences of right ascension of the Sun and planet; they have been corrected for refraction, parallax, and for the motion of the Sun; and are compared with the Elements given in the *Nautical Almanac*.

Durham Sid. T.	Green. M.T.	No. of Wires obsrd.	Limb of ☿ obsrd.	Difference of R.A. of Centres, ☿ − ⊙	Computed from N.A.	C − O.
h m s	h m s			s	s	s
11 1 42·9	20 8 42·0	3	I.	−42·338	−42·432	−0·094
11 9 42·7	20 16 41·2	3	II.	−45·188	−45·354	−0·166
11 15 21·7	20 22 19·0	3	II.	−47·442	−47·414	+0·028
11 34 1·2	20 40 55·5	2	I.	−54·044	−54·222	−0·178
11 37 31·1	20 44 24·7	3	I.	−55·267	−55·498	−0·231

The following differences of declination were also observed and are similarly corrected and compared with the *Nautical Almanac*:—

Durham Sid. T.	Green. M.T.	No. of Measures.	Limb observed. ☿ ⊙	Difference of Decl. of Centres.	Computed from N.A.	C − O.
h m s	h m s			′ ″	′ ″	″
11 25 41·2	20 32 36·7	3	N.L. − S.L.	−7 41·11	−7 37·46	+3·65
11 27 26·5	20 34 21·8	2	S.L. − S.L.	−7 35·73	−7 32·94	+2·79

An attempt was also made to measure the diameter of *Mercury*. The micrometer employed was a double image micrometer of Mr. Airy's original construction. The value of its screw was determined in March 1868, and found to be 18″·693, a value closely agreeing with that determined some years ago by the Rev. R. Thomson. The mean of eight measures gives for the diameter 9″·001, which is a somewhat small result. Owing to the rapidity with which the observations were necessarily taken, and the rather unsatisfactory image of the planet, this result cannot be considered of any great value; the individual measures are, however, very fairly accordant.

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On the Transit of Mercury. By W. Lassell, Esq.

1868, Nov. 4, 20½ hours. Observed part of the transit of *Mercury*. Telescope used, a Gregorian of 4·7 inches aperture, power 120. The Sun not visible over the trees until about $19^h\ 50^m$, and then too much disturbed to see anything well. At $20^h\ 20^m$ undulation less, but still the planet and Sun's limb greatly agitated. At $20^h\ 55^m$ settled myself at the telescope to observe last contacts, though from the state of the limbs no *accurate* observation could be expected. The nearest times which it was possible to note were the following:—

	h	m	s
First contact	21	0	12
Planet apparently bisected at	21	1	12
Last contact, best observed of the three ..	21	2	22·8

Times by watch, which was slow for G.M.T. 0 0 25

Assumed places of Ray House, Latitude 51° 31′ 23″

Longitude $2^m\ 49^s{\cdot}3$ west.

Some fine spots were on the Sun, one towards the eastern limb, almost exactly the same size as the planet; but the contrast of shade was so striking as to give an optical proof how very far even the central part of a solar spot is from absolute blackness.

Ray Lodge, Maidenhead,
November 5th, 1868.

On the Transit of Mercury. By F. C. Penrose, Esq.

Thinking that, amongst the numerous and far more important communications which you will receive of the observations of the transit of *Mercury*, there may yet be some value for one made with a small aperture and moderate magnifying power — the value, if any, being to illustrate the question of the transit of *Venus* in 1769 — I beg to communicate the following observation:—

Focal length	30in.
Aperture	2¼

Concave single lens eye-piece; magnifying power 70.

I do not doubt that my G.M.T. would be true to half a second. The error could not exceed a second.

The state of the definition fairly good with the power employed, but would not have suited a higher. The Sun's limb boiled all along, but not very violently.

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	G.M.T. h m s
Thread of light interrupted at internal contact	21 0 53·7
Boiling recommenced along the limb after the planet, and 10^s after the notch ceased to be visible	21 2 37·9

Both observations appeared very sharp and sudden, but the last observation was the best.

The time which elapsed between these observations was

$$1^m\ 44^s{\cdot}2$$

the calculated duration appears to be

$$2^m\ 34^s{\cdot}8$$

and the time of final egress for this station

$$21^h\ 2^m\ 57^s{\cdot}7.$$

It appears, therefore, that irradiation and other circumstances connected with the smallness of the telescope and low power employed, reduced my observed duration by 50^s·6; and this effect was more considerable at internal than at external contact. And this nearer coincidence in the latter case may, perhaps, be accounted for by the diminution of the disturbing effect of the atmosphere on the Sun's rays in the line of vision, by the intervention of the planet.

Wimbledon, Nov. 11, 1868.

On the Transit of Mercury. By James Buckingham, Esq.

On the morning of November 5th, 1868, excellent observations were made on *Mercury* during his transit over the Sun's disk, as from the time of sunrise till the end of the transit there was no trace of a cloud near, but a fine clear sky, and, about half-an-hour after the Sun had risen, the air became very steady. The contacts were both seen on the preceding limb of the Sun, and noted with great exactness by the chronograph at the following times, viz.:—

	h m s	
Internal contact at	11 59 8·61	Sidereal local time.
External contact at	12 1 59·09	„ „

Or in Greenwich mean time:—

	h m s
	21 0 18·77
	21 2 40·85
Duration	0 2 22·08

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The internal contact occurred very suddenly by an apparent elongation of the planet's semi-diameter near the place of contact, when, by the previous appearance of the streak of light of the Sun, I had expected some ten to twelve seconds would have elapsed before the contact would occur: but at the time noted the circularity of *Mercury* was lost at point of contact (when all light of the sun at that part disappeared) as roughly shown in the sketch.

During the transit the image of the planet was beautifully defined and round, absolutely *black* without the slightest *penumbra*. During my observations, twice there existed an unsteadiness, and I noticed the image of *Mercury* expand and contract rapidly; but, during a long steady period, I obtained ten sets of measures of the planet's diameter, with a power of 340 on my 9-inch Equatoreal; the mean of them gave 7″·651, differing 2¼″ from that given in the *Nautical Almanac;* I used the micrometer (parallel wire) on my larger instrument, power 610; the result of five sets of measures differed only 0·3 from the previous (more or less, viz. 7″·618), and I am quite satisfied these measures are approximately true, but the great discrepancy for some moments very much surprised me. Yet, on reflection, I think it quite consistent that the observed diameter of a planet in transit should appear less than the true, owing to the effect of irradiation from the intense light of the Sun appearing to encroach on the dark body; and this supposition, I apprehend, is consistent, because so soon as the actual limb of the planet had really covered the true limb of the Sun all irradiation existing up to that moment (and thereby reducing the apparent diameter of the planet) would cease, and the *true semi-diameter of the planet* would be apparent and cause the appearance of elongation as described.

	°	′	″	
Observatory	51	29	13	N. Lat.
	0	4	58	W. Long.

I have thought it possible these few remarks may interest the Society, and beg to submit them.

Westmoreland House, Walworth Common.

On the Transit of Mercury. By C. L. Prince, Esq.

The weather this morning was favourable for observing the latter part of the transit of *Mercury* over the Sun's disk.

The definition was unusually bad for some time after sunrise, but at eight o'clock it much improved, and the planet appeared as a circular black spot on the Sun's disk. Its colour was much more intense than the nuclei of the solar spots which were present. I observed nothing worthy of note till the planet was

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approaching internal contact, when a sudden elongation or lengthening out of the planet's horizontal diameter, almost to a point, occurred; and internal contact appeared to happen sooner than might have been expected from the previously circular form of the planet. This distortion of the figure of the planet continued till its centre was upon the Sun's limb; when it ceased. During the remainder of the egress, even to the moment of last contact, no distortion whatever occurred. The telescope I employed was my Equatoreal of 6·8 inches aperture and 12 feet focal length. The magnifying power was 210 in a Dawes solar eyepiece, with blue glass shade. The times of contact were as follows:—

	h.	m	s	
The Internal Contact took place at	9	0	54·1	Local Mean Time.
The External ,, ,,	9	3	11·6	,,

My Observatory, in time, is 24″ East of Greenwich.

Uckfield Observatory, Nov. 9, 1868.

On the Transit of Mercury. By G. Williams, Esq.

Having been more fortunate than some of my neighbours in seeing the recent transit of *Mercury*, I venture to send a drawing and some measures I obtained of the planet's position on the Sun's disk.

A parallel wire micrometer was made use of, the Sun's limb being made to run along the upper moveable wire; the difference in time of transit between the Sun's preceding limb and the planet over the fixed wire was then taken; the time at once noted and the lower moveable wire brought up as quickly as possible to the planet's position.

The revolutions of the micrometer-screw were reduced to seconds in the usual way, and the difference in the times of transit converted into seconds of arc, and corrected by the cosine of the declination.

I once fancied I saw a ring of ruddy coloured light round the planet, but, for the greater part of the transit, nothing of the kind was visible.

There was no apparent elongation, pear shape, or black drop at the egress of the planet; but the boiling of the limb, which was considerable, may account for the absence of these appearances.

The telescope used was the equatoreal refractor, 4·25 inches aperture and 63 inches focus, by Messrs. Cooke and Sons; powers 71 to 140.

2 *Devonshire Road, Prince's Park, Liverpool,*
Nov. 11, 1868.

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The figure which accompanies the foregoing Note shows the following three positions of the planet in regard to the Sun's disk, viz.—

	h m s	x =	y =
At	20 15 54	288·67	474·27
	20 26 56	259·81	496·90
	20 54 26	121·24	570·88

where x is the distance in the direction of a parallel of declination, and y the distance in the direction at right angles to this, from an origin outside the Sun's disk, such that in regard to it the co-ordinates of the Sun's centre are

$$x = y = \text{Sun's radius} = \tfrac{1}{2} \cdot 1941''{\cdot}4;$$

and further, that at egress the times of internal contact and of external contact were $21^h\ 0^m\ 18^s$ and $21^h\ 2^m\ 26^s$ respectively.—Ed.

On the Transit of Mercury. By Capt. Noble.

In common, I suppose, with every possessor of a telescope in England, I observed the transit of *Mercury* on the morning of Thursday, November the 5th.

Most unfortunately, through a circumstance over which I had no control, I had to withdraw my eye from the instrument at the instant of last internal contact; but, on re-applying it to the telescope immediately after this had taken place, I then noticed that the planet seemed to be somewhat drawn out, or *squared*, as roughly shown in my sketch; the double inversion of which arises from my observation having been made with the "Hodgson" reflector. I watched *Mercury* across the Sun's disk with a power of 74, but exchanged this for one of 154, after internal contact had taken place, and with this observed the egress. The last external contact or final emersion occurred at

$$12^h\ 2^m\ 30^s \text{ L.S.T.} = \text{Nov. } 4,\ 21^h\ 3^m\ 0^s{\cdot}99 \text{ L.M.T.}$$

a determination which I scarcely think is 1 second $\pm$ in error. I employed, as usual, my 4·2-inch Equatoreal of 61 inches focal length in making the observation.

Forest Lodge, Maresfield, Sussex,
12 Nov. 1868.

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The transit of *Mercury* was observed at Rome by the Rev. Father Secchi, thus:—

1868, Nov. 5, Rome M.T.

	h	m	s
Ring broken at	21	50	5·91
Planet out at	21	52	27·46

Four Photographs of the Sun and *Mercury*, taken at Ely on November 5, communicated by the Rev. Prof. Selwyn, were exhibited at the Meeting. Prof. Selwyn writes:—

"At the taking of the third of the series, the planet was close to the edge, and was visible on the negative; but I cannot be quite sure of his appearance in the print. We ought to have given a larger aperture, or longer exposure, to insure a good representation of *Mercury* in the comparative darkness of the Sun's limb, but it was not thought of. The fourth is given, at a later hour, to remove all doubt as to the nature of the round spot representing *Mercury*."

On the Appearance of Mercury at its Transit, November 5, 1868. By William Huggins, F.R.S.

As buildings intercepted the view of the Sun from my observatory before $8^h\ 30^m$, I determined not to attempt any measures of *Mercury*, but to devote the short time at my disposal to a careful scrutiny of the appearance which the planet might present when seen upon the Sun.

The Sun's edge was a little tremulous from atmospheric agitation, but the solar surface was so well defined that the bright granules of which it is composed could be distinctly seen. The planet appeared as a well-defined, round, black spot. Whilst carefully examining the immediate neighbourhood of the spot for the possible detection of a satellite, I perceived that the planet was surrounded with an aureola of light, a little brighter than the solar disc. The breadth of the luminous annulus was about one-third of the planet's apparent diameter. The aureola did not fade off at the outer margin, but remained of about the same brightness throughout, with a defined boundary. The aureola was not sensibly coloured, and was only to be distinguished from the solar surface by a very small increase of brilliancy.

Almost at the same moment that I first perceived the surrounding annulus of light, I noticed a point of light nearly in the

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centre of the planet. This spot of light had no sensible diameter with the powers employed, but appeared as a luminous point.

These phenomena, the aureola and the spot of light on the planet, were distinctly visible as long as the transit continued.

The whole aperture, 8 inches, of the telescope was in use, with a prismatic solar eye-piece, and powers of 120 diameters and 220 diameters. A sliding wedge of neutral tint glass allowed the apparent brightness of the Sun to be rapidly varied, but with all parts of the wedge brought before the eye, the phenomena described above continued to be visible.

It is of importance to state that the aureola was seen most plainly when the darkest part of the wedge was before the eye. This observation is scarcely what would have been expected on the supposition that the aureola is merely an optical or ocular phenomenon, for the conditions when the Sun was greatly darkened were favourable for the discrimination by the eye of the existence of a small difference of illumination; but they were, in the same degree, unfavourable for a mere optical effect produced by contrast. I am sorry that I omitted to notice if the definition of the granular structure of the solar surface was equally good, immediately round the planet where the aureola appeared.

I watched carefully the bright point upon the planet as *Mercury* was passing off the Sun, to ascertain if this luminous spot could be seen after the planet had become invisible. I kept it steadily in view until the part of the planetary disc, where the point of light was situated, reached the Sun's limb, I then ceased to see it. This observation must be regarded as negative merely, and not as proving the invisibility of the luminous point when the planet had passed off the Sun, for so small a point of light might be easily overlooked when the form of the planet was no longer visible to serve as a guide to the eye.

It may be well to state that at the time of making these observations, the appearances described by Schröter, Moll, and others, which I had not read for some years, were absent from my memory. I was not looking for these appearances, and it was some little time before I would believe in their reality.

If *Mercury* be surrounded by a transparent atmosphere the solar rays would be bent in on all sides and would cross in front of the planet, but would then proceed in directions far too much removed from the line joining the Sun, *Mercury*, and the Earth, to be received by the telescope. Such an atmosphere, in consequence of its power of turning aside the solar beams incident upon it, should appear darker than the solar surface.

Similar phenomena have been observed at some former transits. A sort of ring of faint light was seen by Plantade at the transit of 1736; also by Proserpin; also by Flaugergues in 1786, and in 1789, and 1799. He calls it "un anneau lumineux." Mechain, Messier, Fritsch and Seyffler observed a similar phenomenon. It is also described by Schröter and Harding during the transit of 1799. In 1832 Dr. Moll saw it as "a nebulous ring of

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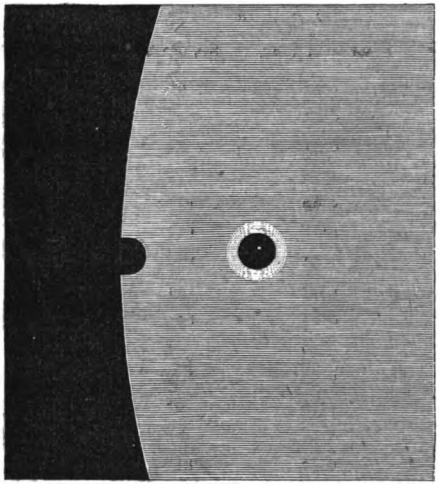
a darker tinge approaching to a violet colour." Some of these observers appear to have considered the aureola to be slightly brighter, and others as in a small degree darker than the Sun.

The other phenomenon, that of a bright spot on the planet, has been seen at former transits. At the same time that Schröter, in 1799, perceived the ring, he saw on the body of the planet a small bright spot, which was seen independently by Harding. At a later period of the day, Schröter saw the luminous speck, sometimes on one part of the disc and sometimes on another, being unable to keep it constantly in view.

A bright spot, not in the centre of the disc, was seen by Dr Moll at the transit of 1832.*

Professor Powell has proposed to account for the bright spot as a phenomenon depending on the diffraction of light.† As, however, in all the observations the spot was not seen in the centre of the planet's disc, this explanation seems to be scarcely applicable.

The following appearance was noticed almost immediately after the planet's disc came up to the Sun's limb. The spot appeared distorted, spreading out to fill up partly the bright cusps of the Sun's surface between the planet's disc and the sun's limb. This appearance increased as the planet went off the Sun, until when the disc of the planet had passed by about one-third of its diameter, it presented the form represented in the diagram, in which the



* See Dr. G. Moll on Transit of 1832, *Mem. R. Astron. Society*, vol. vi. p. 116; also, Breen, *The Planetary Worlds*, p. 138. I have been unable to refer to Schröter's original observations in the *Hermographische Fragmente*. This work is not in the library of the R. Astronomical Society, nor in that of the Royal Society.

† Prof. Powell on Irradiation: *Mem. R. Astron. Society*, vol. xviii. p. 69.

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margin of the disc from points at the end of a diameter parallel to the Sun's limb, instead of continuing its proper curve, appeared to go in straight lines up to the limb, thus entirely obliterating the cusps of light, which would otherwise have been seen between the planet and the limb. In the diagram the aureola and the bright spot are not repeated in the figure of the planet on the Sun's limb.

A Determination of the Constant of Nutation from the Observations of Polaris in R.A. made at the Royal Observatory, Greenwich, from 1851–1867. By E. J. Stone, Esq.

In the June number of the *Monthly Notices* of the Royal Astronomical Society appears an abstract of a paper of mine on "A Determination of the Constant of Nutation from the Observations in N.P.D. of *Polaris*, *Cephei* 51, and *δ Ursæ Minoris*, made with the Transit Circle of the Royal Observatory, Greenwich, 1851–1865." In the present paper I have discussed the observations of *Polaris* in R.A. made with the Transit Circle of the Greenwich Observatory, from 1851 to 1867, both inclusive.

The observations, as extracted from the yearly Catalogues, are given in Table I.

Mean Right Ascensions of *Polaris* for Jan. 1, extracted from the Annual Catalogues.

Year.	Fraction of Year.	No. of Obs.	R.A. h m s
1851	0·46	99	1 5 19·02
1852	0·43	128	1 5 36·75
1853	0·61	76	1 5 54·65
1854	0·53	169	1 6 12·98
1855	0·60	110	1 6 30·86
1856	0·48	113	1 6 49·36
1857	0·52	150	1 7 7·37
1858	0·47	191	1 7 25·90
1859	0·55	155	1 7 44·23
1860	0·46	92	1 8 3·17
1861	0·50	116	1 8 21·50
1862	0·59	67	1 8 40·52
1863	0·48	78	1 8 59·63
1864	0·47	112	1 9 19·08
1865	0·52	110	1 9 38·06
1866	0·53	93	1 9 57·87
1867	0·64	77	1 10 17·34

The clock-errors from 1851 to 1855 were determined from the right ascensions of fundamental Stars, as given in the Greenwich

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twelve-year Catalogue: those from 1856 to 1861 from the R.A.'s of the Greenwich six-year Catalogue: those from 1862 to 1867 from the R.A.'s of the Greenwich seven-year Catalogue. To bring the system to uniformity, the R.A.'s from 1851 to 1855 will be increased by $+0^s.013$: those from 1856 to 1861 by $+0.004$.

The Nutation constant adopted during the years 1851–1856 was $9''.25$. The constant from 1857 to 1867 was $9''.224$. The following corrections will be applied for the effect of this difference of adopted constants:—

	s
To the Results of the Year 1851	+0.018
1852	−0.004
1853	−0.029
1854	−0.045
1855	−0.060
1856	−0.066

The following are the corrections applied to the results of each year to allow for the precessional motion in R.A. from the year 1859. The Pulkova constants have been used:—

	m	s
Correction to Result for 1851	= 2	24.397
1852	= 2	6.766
1853	= 1	49.018
1854	= 1	31.151
1855	= 1	13.165
1856	= 0	55.058
1857	= 0	36.829
1858	= 0	18.477
1859	= 0	0.000
1860	= 0	18.604
1861	= 0	37.336
1862	= 0	56.195
1863	= 1	15.184
1864	= 1	34.306
1865	= 1	53.561
1866	= 2	12.951
1867	= 2	32.477

Then, assuming

Proper Motion in R.A.	$= 0^s.1148 + p$
True Constant of Nutation	$= 9''.224 + y$
Mean R.A. of Polaris, 1859, Jan. 1	$= 1^h\,7^m\,44^s + x$

we have the following equations of condition for the determination of x and y:—

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					w.
1851	x	$+ 0{\cdot}680\, y$	=	$0{\cdot}366 + 8\, p$	99
1852	x	$- 0{\cdot}141\, y$	=	$0{\cdot}329 + 7\, p$	128
1853	x	$- 1{\cdot}119\, y$	=	$0{\cdot}341 + 6\, p$	76
1854	x	$- 1{\cdot}720\, y$	=	$0{\cdot}673 + 5\, p$	169
1855	x	$- 2{\cdot}315\, y$	=	$0{\cdot}437 + 4\, p$	110
1856	x	$- 2{\cdot}543\, y$	=	$0{\cdot}700 + 3\, p$	113
1857	x	$- 2{\cdot}523\, y$	=	$0{\cdot}433 + 2\, p$	150
1858	x	$- 2{\cdot}232\, y$	=	$0{\cdot}496 + p$	191
1859	x	$- 1{\cdot}623\, y$	=	$0{\cdot}234$	155
1860	x	$- 0{\cdot}935\, y$	=	$0{\cdot}455 - p$	92
1861	x	$- 0{\cdot}035\, y$	=	$-0{\cdot}062 - 2\, p$	116
1862	x	$+ 0{\cdot}920\, y$	=	$-0{\cdot}019 - 3\, p$	67
1863	x	$+ 1\,608\, y$	=	$-0{\cdot}013 - 4\, p$	78
1864	x	$+ 2{\cdot}212\, p$	=	$+0{\cdot}200 - 5\, p$	112
1865	x	$+ 2{\cdot}587\, y$	=	$-0{\cdot}190 - 6\, p$	110
1866	x	$+ 2{\cdot}652\, y$	=	$+0{\cdot}115 - 7\, p$	93
1867	x	$+ 2{\cdot}369\, y$	=	$-0{\cdot}055 - 8\, p$	77

Multiplying each equation by the corresponding value of $\sqrt{w}$, and reducing the equations by the method of least squares, we have for the determination of x and y the equations —

$$1936\ x - 866{\cdot}518\, y = 581{\cdot}466 + 935\ p$$
$$-866{\cdot}518\, x + 6986{\cdot}703\, y = -980{\cdot}016 - 11340{\cdot}24\, p$$

From which

$$x = + 0{\cdot}252 - 0{\cdot}26\,.\,p$$
$$y = - 0{\cdot}109 - 1{\cdot}66\,.\,p$$

or Mean R.A. of *Polaris*, 1859, Jan. 1,

$$= 1^{h}\ 7^{m}\ 44^{s}{\cdot}252 - 0{\cdot}26\, p$$

$$\text{Constant of Nutation} = 9''{\cdot}115 - 1{\cdot}66\, p$$

If we adopt for all the Proper Motions the values given in "Gould's Standard Places of Fundamental Stars, *second edition*," we have for

Proper Motion in R.A. of Polaris	$= 0{\cdot}^{s}1148 + p$
N.P.D. of Polaris	$= -0{\cdot}004 + \delta p$
N.P.D. of Cephei 51	$= +0{\cdot}048 + \delta c$
N.P.D. of δ Ursæ Minoris	$= -0{\cdot}043 + \delta m$

The following will then be the resulting values of the constant of Nutation : —

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(1) $N = 9''.115 - 1.66\ p$ from the R.A. of Polaris
(2) $N = 9.109 + 6.1\ \delta p$,, N.P.D. of Polaris
(3) $N = 9.143 - 3.6\ \delta c$,, N.P.D. of Cephei 51
(4) $N = 9.214 + 1.75\ \delta m$,, N.P.D. of δ Ursa Minoris.

Giving (1) and (2) twice the weights of (3) and (4), we have for the final value of the constant of Nutation,

$$9''.134 - 0.55\ p + 2.02\ \delta p + 0.29\ \delta m - 0.6\ \delta c.$$

Occultation of Stars by the Moon, 8 *Sept.* 1868.
By J. Joynson, Esq.

		h	m	s	
71 Tauri	Dis.	11	31	17·7	G.M.T.
β^2 Tauri	Dis.	12	40	16·5	
	Re.	13	41	25·5	
β^1 Tauri	Dis.	12	46	15·4	
	Re.	13	36	7·1	
81 Tauri	Dis.	13	45	6·8	
	Re.	14	18	8·6	
1391 B.A.C.	Dis.	14	10	21·8	
85 Tauri	Re.	14	57	20·7	

The occultation of *α Tauri* could not be observed owing to dense cloud.

Waterloo, near Liverpool.

Mr. Chambers has communicated the following Errata in his "Catalogue of Binary Stars," *Monthly Notices*, vol. xxviii. pp. 174–179.

Part I.

The columns of Epoch should be headed 1800 +, *not* 1880 +
No. 4. 34 *Andromedæ* should be 36, and the Mag. of A, 6, *not* 4.
No. 31. The Epoch should be '63·1, *not* '66·1.
No. 80. Struve's synonym should be 3127, *not* 2137.

Part II.

No. 19. The Epoch should be '64·7, *not* '65·7.
No. 31 is misplaced in R.A.; it should follow 28.
No. 42. The Decl. should be 21° 36″·3, *not* 21° 26″·3.
No. 43. The Magnitude of B should be $7\frac{3}{4}$, *not* $8\frac{1}{4}$.

Bickley, Kent, August 6th, 1868.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIX. *December* 11, 1868. No. 2.

Admiral MANNERS, President, in the Chair.

Rev. C. Gribble, British Embassy, Constantinople;
Dr. G. Neumeyer, 10 Gower Street; and
David Vines, Esq., Kingswood, Bristol,

were balloted for and duly elected Fellows of the Society.

Rec. read for discussion Jan. 1864.

On the Preparatory Arrangements which will be necessary for efficient Observation of the Transits of Venus in the years 1874 and 1882. By George Biddell Airy, Astronomer Royal.

On two occasions (*Monthly Notices*, 1857, May 8, and 1864, June 10) I have called the attention of the Society to the Transits of *Venus* across the Sun's disk, which will occur in the years 1874 and 1882; and have pointed out that, for determination of the difference between the Sun's parallax and the parallax of *Venus*, the method by observation of the interval in time between Ingress and Egress at each of two stations at least, on nearly opposite parts of the Earth (on which method, exclusively, reliance was placed in the treatment of the observations of the Transit of *Venus* in 1769), fails totally for the Transit of 1874, and is embarrassed in 1882 with the difficulty of finding a proper station on the almost unknown Southern Continent.

The publication of M. Le Verrier's new Tables of *Venus*, and

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of Mr. Hind's inferences from them as to the points of the Sun's limb at which the Ingress and Egress will take place in each Transit (which inferences I have in part verified), has induced me again to examine the whole subject. And, without giving up the hope of using the observation of interval between Ingress and Egress at each of two stations in 1882, I have come to the conclusion (from all the information which has reached me) that it will be unsafe to trust exclusively to the chance of securing observations on the Southern Continent; and that, while observations are by all means to be attempted in that manner, it is also very desirable to combine with them observations of the same phenomenon (at one time the Ingress, at another time the Egress), made at nearly opposite stations whose longitudes are *accurately* known, and recorded in *accurate* local time. This principle being once admitted, the Transit of 1874 is, or may be, as good for observations of that class as the Transit of 1882; and the selection of localities for the observations must be made with equal care for the two Transits.

Mr. Hind's calculations for the points of Ingress and Egress are contained in a paper in the *Comptes Rendus* of the French Academy for 1861, July 22. My valued friend has been so much accustomed, in the admirable calculations issued from his office, to describe phenomena as seen in an inverting telescope (a method of exhibition which I could wish entirely to banish) that he has inadvertently given the elements of Ingress and Egress on that supposition, but without explanation of it. Correcting for this peculiarity, the elements to be used for discussion of observation-stations are, with sufficient approximation, the following:—

Transit of Venus, 1874, *Dec.* 8. *Sun's Declination* 22° 35′ *S.*
Ingress at $14^h\ 0^m$ Greenwich Mean Solar Time.
Position on Sun's Limb 131° from South through East towards North.
Egress at $18^h\ 12^m$ Greenwich Mean Solar Time.
Position on Sun's Limb 160° from South through West towards North.

Transit of Venus, 1882, *Dec.* 6. *Sun's Declination* 22° 44′ *S.*
Ingress at $2^h\ 5^m$ Greenwich Mean Solar Time.
Position on Sun's Limb 35° from South towards East.
Egress at $8^h\ 2^m$ Greenwich Mean Solar Time.
Position on Sun's Limb 66° from South towards West.

With these data, a common terrestrial globe can be adjusted for each place, on the supposition that the Sun is vertically above the globe, and therefore that the globe-horizon defines those parts of the earth to which the phase will be visible. If a point be taken on the globe-horizon corresponding to the "Position on Sun's Limb," the terrestrial station thus indicated is that at which Parallax has its maximum effect in apparently displacing

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Venus towards the Sun's centre; and the opposite station is that where the maximum apparent displacement from the Sun's centre occurs. The effect at any other station is proportional to the cosine of its distance, in arc of great circle of the Earth, from the maximum station.

The Maps which I attach to this paper have been prepared by taking advantage of the principle which I have described. They do not profess any extreme accuracy, but they will, I trust, be found abundantly accurate for the purpose for which they are prepared. All are on the same scale except the first, whose scale is $\frac{3}{4}$ that of the others. It will be remarked that the local elements, which at any station are important, are, first, the "Factor of Parallax," or the cosine of angle above mentioned, on which depends the theoretical value of the observation; second, the "Sun's elevation above the horizon," on which the chance of good observation will greatly depend. These are defined upon the maps; for the first, by the station's place among the semicircular curves; and for the second, by the station's place among the parallel lines. A point surrounded by a circle, upon the horizon-line, indicates the place at which the effect of Parallax is maximum.

It will be remarked that, in this mode of observing the Transit, the Ingress is a phenomenon complete in itself, requiring to be observed at nearly opposite sides of the Earth; namely, at one side where it is apparently accelerated by Parallax, and at another side where it is apparently retarded by Parallax. The two maps corresponding to these two tracts of observation will be placed in the bound volume, face to face. In like manner, the Egress is a phenomenon complete in itself, also requiring two districts of observation on nearly opposite sides of the Earth, different from those used for the observation of Ingress, and therefore requiring two special maps, and these in like manner will be bound face to face.

I can now proceed to discuss the individual maps with reference to the selection of stations for the observation of the several phenomena.

Transit of Venus, 1874, Dec. 8.

Map No. 1.—*Stations for observing the Ingress as accelerated by Parallax.*

Owhyhee and the neighbouring islands are excellent. The factor of parallax is about 0·92, and the Sun is at nearly two hours' elevation. There is English society at Woahoo. These islands are just within the tropics. For use of this station the absolute longitude must be accurately determined.

At the Marquesas Islands the Factor of Parallax is 0·7, and the

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Sun is nearly as high as at Woahoo. Our neighbours across the Channel have, from the time of Louis XIV., taken an honourable lead in scientific enterprise of every class. I trust that we may rely on them for accurate determination of longitude at Marquesas, and for accurate observation of the Ingress in 1874.

The desert Aleutian Islands can scarcely be recommended, although the factor 0·8 for the westernmost of them, where the Sun is highest, is favourable. But it is very probable that the Russians will soon have established telegraphic communication with the mouth of the Amoor, by which its absolute longitude will be accurately determined; and though the factor is only 0·57, the Sun is 15° high, and the station will be valuable.

On the whole, if the British Government will undertake the accurate determination of longitude of Woahoo, and the careful observation of Ingress there in 1874, we may consider that good provision is made for the Accelerated Ingress.

Map No. 2.—*Stations for observing the Ingress as retarded by Parallax.*

The best station, as referred to the test of numbers, is Kerguelen's Island, where the factor is 0·91, and the Sun is 25° high. This island is emphatically known as "The Island of Desolation." I know not whether its character is so repulsive, or its utility as a zero of longitude so small, as to make our nautical authorities unwilling to determine its longitude, and to station observers there in 1874. If these difficulties are not thought too great, it will be an excellent position. At Crozet's Islands the factor 0·98 is very favourable, but the Sun is rather low (10° altitude).

The next stations in order of merit are Rodriguez, Mauritius, and Bourbon. Mauritius possesses this claim, that it will be a fairly good station, though not so good as Bourbon, in 1882; in 1874, as well as in 1882, it has this disadvantage, that the Sun will be low. If only one longitude can be determined in this chain of islands, it ought to be that of Mauritius; if two can be determined, they ought to be those of Rodriguez (for 1874) and Bourbon (for 1882).

At Madras and Bombay the factors, 0·47 and 0·44, are small; but the value of either station does not depend entirely on its simple factor, but upon the sum of its factor with the factors at the stations in Map No. 1. These two Observatories, with well-known longitudes, will prove very useful stations.

With the assistance which we may hope to receive from the British Government, we may consider the observation of the Retarded Ingress as well secured.

Map. No. 3.—*Stations for observing the Egress as accelerated by Parallax.*

Excluding from consideration the Southern Continent, as not to be entertained in our thought without the most absolute

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necessity, the stations in order of merit are the Auckland Islands, Canterbury, Wellington, and Auckland, in New Zealand (factors ranging from 0·83 to 0·77), Norfolk Island (0·66), Melbourne and Sydney (0·6). I omit Chatham Island, where the Sun is rather low. The existence of the Observatories at Melbourne and Sydney makes the observation of the Accelerated Egress almost secure, although, in confirmation, I should much desire to have one station at least on the New Zealand group.

Map No. 4.—*Stations for observing the Egress as retarded by Parallax.*

The stations which are favourable for this observation are almost entirely on Russian and Turkish territories. At none of them is the factor less than 0·84; and we have, therefore, only to consider the elevation of the Sun, leaving to the national governments to estimate the facilities or difficulties depending on the locality, the climate, or the season. Any station either to the east or to the west of the Lower Caspian will have the Sun well elevated. Omsk, Orsk (whose longitude has been determined with peculiar care), Astrakhan, Erzeroum, Aleppo, Smyrna, and Alexandria, have the Sun sufficiently high. At Tobolsk, Perm, Kazan, Kharkov, Odessa, Constantinople, and Athens, the Sun will be rather low, and at Moscow it will be on the horizon. We may with the utmost confidence leave the selection of the stations, the determination of longitude, and the observation of the phenomenon, to our Russian friends. One station, however, ought specially to be considered as being, for this purpose, in British hands, namely, Alexandria. It appears not improbable that we may soon have very direct telegraphic communication with Alexandria; but failing this, I trust that no efforts will be wanting to determine accurately its longitude; a longitude which was in the survey of Admiral Smyth, and which always must be, the zero of longitude in the Levant. This being ascertained, Alexandria would probably be the best of all the stations for observation of the Retarded Egress.

Transit of Venus, 1882, Dec. 6.

First, by the method of absolute Longitudes.

Map No. 5.—*Stations for observing the Ingress as accelerated by Parallax.*

Omitting for the present all allusion to the Southern Continent, it will be seen that the best station is Kerguelen's Island, its factor being 0·98, and the Sun's elevation (12°) probably sufficient. This circumstance, in addition to its value as explained in the discussion of Map No. 2, renders it well worthy of attention. At Crozet's Islands the factor is 0·9, and the Sun's elevation 23°; abstractedly it is preferable to Kerguelen's Island,

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but not in quite so great a degree as that in which Kerguelen's Island is superior in Map No. 2. The next in value are Bourbon and Mauritius, with factor about 0·78, the Sun being higher at Bourbon. On comparing these qualifications with those remarked under Map No. 2, the reasons will be evident for my recommendation that either the longitude of Mauritius or the longitudes of Bourbon and Rodriguez should be determined.

At the Cape of Good Hope the factor is about 0·62, and the observation there will be valuable.

The satisfactory observation of the Accelerated Ingress requires, however, some longitude-determinations.

Map No. 6.—*Stations for observing the Ingress as retarded by Parallax.*

Every city near the seaboard of the United States of America, and every important city of Canada, commands this phenomenon most favourably. The lowest factor is 0·95, and the smallest elevation of the Sun is 12°. The utmost reliance may be placed on the zeal of our American brethren for observing the Ingress. As great facility exists for determining the absolute longitude of any place within the range of American telegraphs (Harvard having been accurately referred to Greenwich), it is unnecessary to look further. Otherwise it might be remarked that Bermuda, Jamaica, and the West Indian Islands, and both sides of Central America, are excellent stations, but requiring determinations of longitude.

Map No. 7.—*Stations for observing the Egress as accelerated by Parallax.*

All the American stations mentioned in the last paragraph, from Halifax to New Orleans, and Bermuda and the West Indian Islands, are well situated for this observation, the factors being near 0·85, and the Sun's altitude varying from 4° at Halifax to 32° at New Orleans and Jamaica. The coast of South America also is favourable, from its union with the Isthmus to the harbour of Rio de Janeiro. It is believed that efforts have been made for exact determination, in a nautical sense, of the longitude of Rio; it may now be desirable to give to that longitude the utmost accuracy.

Map No. 8.—*Stations for observing the Egress as retarded by Parallax.*

Omitting for the present the Southern Continent, this observation will be amply secured by the Observatories of Sydney and Melbourne, where the factor is 0·96, and the Sun's elevation 12° to 14°. If, however, the longitudes of the New Zealand stations can be ascertained, they, with factor 0·8 and Sun's elevation 32°, will form a valuable addition.

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Second, by the Method of interval between Ingress and Egress.

On comparing Maps Nos. 6 and 7, it will be seen that the North American localities supply, in a manner which leaves nothing to be desired, the demand for stations, at which the Ingress is retarded and the Egress accelerated, or the whole interval is diminished, by Parallax.

With these, it is necessary to combine one or more stations, at which the Ingress is accelerated and the Egress retarded, or the whole Interval is increased, by Parallax. On examining Maps Nos. 5 and 8, it will be seen that the only possible method of responding to this demand is by the selection of stations on the Antarctic Continent, in which the observation will be made when the Sun is nearly below the Pole.

In so far as the coast of the Antarctic Continent follows nearly a parallel of latitude, the best position for a station is at 7^h east longitude. The factors would be, for Ingress and Egress, about 0·95 and 0·68. The Sun would be, at each station, about three hours from the sub-polar meridian. But its elevation above the horizon would scarcely exceed 4°, and any alteration of the longitude, with the view of increasing the elevation at one phenomenon, would diminish it at the other.

Advantage may, however, be taken of the deep southern inlet discovered by Sir James Ross, to the western side of which is given the name of South Victoria. If a station can be established in latitude exceeding 72° S., it will be preferable, for observation of Ingress, to the station in 7^h longitude, and if the expedition could be pushed on to an observing place in the neighbourhood of Mounts Erebus and Terror, that position would be greatly preferable. For observation of Egress, it is manifestly far superior ; the Sun's altitude being about 27°. The factors for the two observations are respectively about 0·78 and 0·58.

The decision on the choice to be made between these two stations, and the judgment on the facility, or even the possibility, of using either of them, must rest with persons who have had some familiarity with Polar, and if possible with South Polar voyages.

In partial correction of some small inaccuracies in these remarks, it may be observed that,—

The Ingress, as viewed from the Earth's centre, is always a few minutes earlier, and the Egress always a few minutes later, than is supposed in the maps.

As affected by Parallax, the phenomenon is always retarded with ascending Sun and accelerated with descending Sun.

As referred to Apparent Solar Time, the phenomena are slightly retarded.

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The only phenomena which are critically affected by these corrections are those of Maps Nos. 4 and 5, and in both the circumstances of solar elevation are rendered more favourable.

There may be a cause of uncertainty, in the observation-elements on which M. Le Verrier's Tables of *Venus* are founded, arising from the unsatisfactory form in which the observations of the planet were recorded in Bradley's time. At a critical Inferior Conjunction, when *Venus* was apparently a very large body, very deeply hollowed, it is impossible to say whether the limb was observed or whether an attempt was made to observe the centre. In my reductions of the planetary observations I inclined to the former idea. Subsequently M. Le Verrier adopted the latter. The tables are, at this time, very accurate; and it may prove that M. Le Verrier's interpretation was correct. Any uncertainty, however, of this kind, makes it desirable to avoid observations of the Ingress or Egress very near to the horizon.

It appears, from this discussion, that the Absolute Longitudes required for the observation of the two Transits, the determination of which may be considered as a duty of the British nation, are the following:—

Alexandria.
Stations in New Zealand.
Woahoo, or some other point in the Sandwich Islands.
Kerguelen's Island, or Crozet's Islands (Kerguelen's being slightly preferable.)
Mauritius, or the two islands Rodriguez and Bourbon (which would be preferable).

I now proceed to remark on the methods by which these longitudes may be determined.

For Alexandria,—we may probably rely on the transmission of Greenwich Time, either by sending numerous chronometers in the steam ships which make regular voyages between the British ports and Alexandria, or by galvanic signals through the desired direct telegraph-communication. The saving of time and labour by the latter method would be so great that, if there is any reasonable prospect of soon forming that communication, it would be best to wait for it. The Local Time may be obtained by instruments sent for the purpose, either a Transit Instrument for meridian transits, or an Altazimuth for extra meridian zenith distances. Where the labour of computation, as for a limited object, is not worth taking into account, I prefer the Altazimuth. The largest portable instrument, with circles about sixteen inches in diameter, and with four microscopes for reading the vertical circle (two microscopes being insufficient for eliminating the effect of oval form) should be employed; an instrument of this class now forms

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part of the equipment of the Royal Observatory of Greenwich. In every expedition, however, I would send instruments of both classes. It is unnecessary to particularise clocks and chronometers, &c.

For the New Zealand stations, I should trust that connexion by chronometers might be made with Sydney and Melbourne.

For Woahoo and Kerguelen's Island, the circumstances are different. The expense of sending special steamers with chronometers, even from San Francisco to Woahoo, and from the Cape of Good Hope to Kerguelen's Island, would be very great. Here, I think, we must make use of the motions of the Moon. If a hundred transits of the Moon be taken, equally divided between the first and second limb, the probable error of the resulting longitude will be about three times the probable error of a single transit of a star ; or, with liberal allowance for every source of error, it will be below 1^s of time : this accuracy would suffice. A hundred transits might be obtained in a year with the transit instrument, or perhaps in three months by vertical transits with the altazimuth. Through this time, a competent observer must be stationed at the one point. No transmission of chronometers would be required.

The same method, I think, must be adopted for Mauritius, or for Rodriguez and Bourbon.

It would be a question for the Naval Authorities whether the determinations of Absolute Longitude should be preliminary to the observation of the Transits of *Venus*, or whether they should be made about the same time, requiring only one visit of the observer or observers to the station.

For British observation of the Transit of *Venus* in 1874, the number of sets of instruments required (each set including a transit instrument, an altazimuth, a clock, several chronometers, several telescopes, one or two observing tents) would be five (Woahoo, Kerguelen's Island, Rodriguez or Mauritius, Auckland, Alexandria.) For the Transit in 1882, the number required would be three (Kerguelen's Island, Bourbon or Mauritius, Auckland). I suppose here that the Canadian observatories are already sufficiently equipped. The instruments of the 1874 Expedition, thus set free from two stations, would be required at that of which I next speak.

For selection of an observing station on the Southern Continent, I think that it will be best to examine carefully the accounts given by Sir James Ross, and Commodore Wilkes, and their officers, and to decide on the absolute and relative probability of gaining access to the coast on December 6, in the neighbourhood of the Antarctic Circle at 7^h east longitude, or at a much higher latitude on South Victoria ; and also to judge on the possible difference in the transparency of the atmosphere. The choice of station being made, I would not recommend any reconnaissance, but I would propose that an expedition should

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go direct to the selected point, in good time for the observation of the phenomenon. The season is early for south polar expeditions, and any difficulties produced by ice would probably diminish every day. A station being gained, all that is necessary in the way of subsidiary observation is, a few days' observation to give clock rate; then the clock times only of the two phenomena will furnish all that is required.

The first action to be undertaken by the Government is, to procure the stock of instruments; and this ought to be done without delay. An observing plant like that above mentioned is not to be obtained in haste; and the proposed expedition might be entirely crippled by a small negligence on this point. The equipment of ships, and the selection of officers, would probably require much less time.

I have now to advert to the details of the instruments and the observations of the Ingress and Egress.

I trust that my friend Mr. Stone (First Assistant of the Royal Observatory), whose knowledge of the accounts of the Transits of *Venus* in the last century probably exceeds that of any other astronomer, and whose personal experience in the observation of the late Transit of *Mercury* is equal to that of any other, will be induced to attach to this notice his own remarks on the points to which the observer's attention ought specially to be called. We are undoubtingly of opinion that the telescopes ought to be nearly similar, with aperture perhaps of 4 to 5 inches, and used with power perhaps of 120 to 200. When there are several telescopes at one station, some variation among them may be actually advantageous. The protection of the eye from the Sun's bright light, and the neutralization of the atmospheric dispersion, require distinct consideration. From my own experience in the observation of total eclipses, and from my view of *Mercury* in the late transit, I can state positively that much is gained in clearness of outline by the use of an achromatised prismatic coloured glass, adjustible by the observer. The atmospheric dispersion enters very seriously into the question of accurate observation. The very nature of a phenomenon, which is to exhibit in the highest degree the effects of parallax, implies, that the planet must immerge or emerge near the highest or lowest part of the Sun's limb, and that the Sun must not be very high. No circumstances can be imagined in which atmospheric dispersion is more injurious. To meet this, coloured shades should be used which, as examined on a solar spectrum, absorb the ends of the spectrum, leaving the green predominant. I think it probable also that advantage would be gained by attaching to the eye-piece a dispersive prism with its acute angle upwards; on this, however, I propose to make some experiments.

I trust that Mr. De La Rue will be induced to publish, with

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this paper, his proposal in detail, for the use of photography in the register of a transit of *Venus*.

In terminating this communication, I may perhaps remark that the expedition sent into the Pacific for the observation of the Transit of *Venus* in 1769, has always been esteemed as one of the highest scientific glories of Britain in the last century; and I may be permitted to express my hope that it will be surpassed by the efforts which our country will make, with the same object, in the present century. And I close my paper with an extract from the appeal made by my illustrious predecessor, Dr. Halley, forty-five years before the earlier of the Transits then in view. The personal reflection, though applicable with less force to myself, cannot entirely escape my thought:—

"Ac sane vellem diversis in locis ejusdem Phænomeni observationes à pluribus institui, tum ad majorem adstruendam ex consensu fidem, tum ne Nubium interventu frustraretur singularis Spectator, eo spectaculo quod nescio an denuo visuri sunt hujus et subsequentis sæculi Mortales; et à quo pendet Problematis nobilissimi et aliunde inaccessi solutio certa et adæquata. Curiosis igitur syderum scrutatoribus quibus, nobis vitâ functis, hæc observanda reservantur, iterum iterumque commendamus, ut, moniti hujus nostri memores, observationi peragendæ strenue totisque viribus incumbant; iisque fausta omnia exoptamus et vovemus, præprimis ne nubili cæli importunâ obscuritate exoptatissimo spectaculo priventur; utque tandem Orbium cælestium magnitudines intra arctiores limites coercitæ in eorum gloriam famamque sempiternam cedant."

Royal Observatory, Greenwich,
December 3, 1868.

Remarks by Capt. Richards, Hydrographer to the Admiralty.

I can have no hesitation in responding to the President's invitation to offer such observations as may occur to me on the important subject which has occupied the attention of the Society this evening, viz. the approaching Transits of *Venus* over the Sun's disk on December 8, 1874, and December 6, 1882, undoubtedly, in an astronomical point of view, the most important events of the present century.

The Astronomer Royal has, in his accustomed able, lucid, and instructive manner, placed before us the objects to be attained and the means necessary to secure them; it would be quite unnecessary and out of place on my part to take up your time in supplementing these remarks of his.

We can all quite understand and sympathise with the anxiety which the Astronomer Royal must feel as to the precise action which our Government, when the time comes, will take on a question

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of such vital importance to the astronomers of all nations; in ordinary affairs we do not think it necessary to look a-head so far as five years, but, as regards astronomical phenomena, concerning which there can be no uncertainty, and which do not recur but twice in a century, nothing must be left to chance which can possibly be provided for.

Mr. Airy and myself have been in communication on the subject for some months, and he has placed before the Admiralty, in the clearest manner, the arrangements which it will be necessary to make. Although I have no warrant for saying what precise steps will be taken;—indeed it would be impossible for the Admiralty to decide themselves at the present time,—yet I am justified in saying that such arrangements have been made as will insure that the whole question will be brought under their consideration at a sufficiently early period to enable the proper steps to be fully carried out; and it is impossible for any one who has followed the enlightened liberality which the Government of this country has ever adopted in such matters, to fear that Great Britain, which sent the expedition to the Pacific in 1768 for a similar purpose, will be behindhand on this occasion, when the increase in astronomical knowledge and the improvement in astronomical instruments offer such a guarantee of increased success. In regard to the absolute determination of the longitudes of the stations named by the Astronomer Royal as necessary for the observations of 1874, I cannot conceive that any difficulties or obstacles will arise, and the only immediate measures which appear necessary to be taken are in the preparation of the instruments, which undoubtedly should not be long delayed.

In regard to the selection of stations on the Antarctic Continent for the observations of 1882, this is a more serious question, and one involving considerable difficulties, though not, I apprehend, of such magnitude as is conceived by many. It would, however, certainly be necessary to make a reconnoissance in the summer of 1882, in the first place, because we know of no accessible station for a ship either in the longitude of 7 hours east, or in Victoria Land, which was noted by Sir James Ross, both of which places are mentioned by the Astronomer Royal as eligible, and in the second place, because the navigation in these regions is not open until some weeks after the time in which the transit will occur; therefore it would be necessary for the astronomers to pass a winter there, or from January, 1882, until January, 1883; this would be done either on board ship, or by landing the party, perhaps, on Possession Island, Victoria Land, on which Sir James Ross landed in 1841, or on one of the small islands to the south of it. My own opinion, looking to the uncertainty of finding a wintering station for a ship, is that the latter course would be the most feasible, and there would be little doubt of the facility of reaching one or other of these islands with a suitable steam-vessel, making Tasmania or New Zealand the base of operations; doubtless a year passed in this region would be most profitably em-

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ployed in adding to our knowledge of magnetism, and various other branches of physical science.

Remarks by Capt. Toynbee.

The neighbourhood of Prince Edward's Islands, Crozet's, and Kerguelen's Islands, is, so far as my experience goes, subject to a great deal of thick weather, so that, although December may be a favourable season of the year, it would be well to have an observing station on each, in case of one or two being cloudy.

Remarks by Rear-Admiral Ommanney.

Speaking from experiences in Arctic navigation, I consider that to ensure obtaining a good position for the station of the ship on the Antarctic Continent from whence the observations are to be conducted, it is most desirable that a preliminary survey of the coast on the Antarctic Continent should be carried out before the year 1874, confining the survey more particularly to Victoria Sound, the locality for observing the transit.

Sir James Ross, in his memorable voyage, could do no more then than discover the coast line, the ships at his command were such that he could not hazard their safety by making any attempt to delineate the features of the coast, so that our knowledge of the Antarctic Continent remains where it was, of a most limited nature.

It is most important that those who will be engaged on this Expedition shall find, by previous exploration, the position for the ship to winter at, as well as to initiate the Commander and crew with some experience in the navigation of the Antarctic icy seas; without feeling our way, as it were, it would be hazarding this noble work to the risk of failure, were the expedition to proceed direct for making the transit observations with no more knowledge of the Antarctic regions than what we are now confined to.

We have not yet availed ourselves of steam-power for exploring these regions. I am of opinion that with a vessel properly constructed for the service, aided with steam-power, we could with confidence make a thorough search of the coast in Victoria Sound and the adjacent countries during the navigation season, making Van Dieman's Land the head-quarters of the Expedition.

An Expedition of this nature would be attended with many advantages for the interests of science, but most especially in training the officers for the service of the Transit Expedition when the time arrives, and knowing the nature of the position where the ship will have to take up her station, I know from experience how desirable it is that officers should have a special training for icy navigation.

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I fully concur in all that has fallen from the Hydrographer of the Navy, and hope ere long to hear that operations are taking for sending out to explore the Antarctic Seas.

Remarks by Staff-Commander J. E. Davis.

Staff-Commander J. E. Davis had accompanied Sir James Ross on his southern voyage, and had himself landed at one of the suggested positions of the Astronomer Royal, and he believed there would be no difficulty whatever in again effecting a landing at the same place, viz., on Possession Island, off the coast of South Victoria, in latitude 72°; he did not agree with Admiral Ommanney that a reconnoissance and further survey of that coast was desirable, as no sign of a harbour whatever could be discovered by Captain Ross along the coast, the whole of the land being faced with ice as perpendicular as the wall of a room; indeed, so anxious was Captain Ross to find a place in which he could have wintered in the ships, that he declared he "would willingly give his right arm to find one."

With respect to the suggested station in 7^h east longitude he would observe that, although we were not certain of the position and delineation of the land in the vicinity, yet as all the land that had been seen had been described as mountainous, it was natural to suppose that such was its general character, and if so, the mountains would be immediately between the observer and the Sun; and when it was considered that the altitude of the last-named body at the times of Ingress and Egress, on either side of the pole, would only be between 4 and 5 degrees, the chances of seeing the Sun at all would be extremely problematical, and with the station on the coast of South Victoria in latitude 72° in view, at which no such difficulty existed, he thought it would be desirable to give up the position in 7^h east longitude as untenable.

With regard to the period of the season at which the Transit took place it was to be remembered that the 6th of December was so early that no ships had ever reached the Antarctic Circle by that date, and as it would be necessary to arrange the instruments, &c., preparatory to the observation, he might say that the ships ought to be on the spot at least a month before, this would be the 6th of November, a date altogether out of the question; and as the ships could not winter in the South, the party would necessarily have to be landed the year before: but with good huts he had no doubt they could pass the winter very comfortably; they would have a pleasant prospect before them, and plenty of penguins to live on.

In comparison with Kerguelen Island and the Crozet's, Capt. Davis considered the chances of observing the transit—meteorologically speaking—to be greatly in favour of South Victoria.

Capt. Davis stated that he was preparing a paper on the subject of Antarctic discovery and its connexion with the Transit of

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Venus in 1882, for the Royal Geographical Society, in which he should enter more fully into the subject than he could then do.

Remarks by E. J. Stone.

In observing a Transit of *Venus* for the determination of Solar Parallax, there are two phases of the phenomena presented at the Ingress and Egress which should be accurately observed,—

(1.) The real internal contacts;
(2.) The apparent internal contacts.

It will, perhaps, conduce to clearness of explanation if we confine our attention to the phenomena presented at the egress. Similar phenomena are presented, in a reverse order, at the ingress.

The apparent diameter of the Sun is increased, and the apparent diameter of *Venus*, whilst on the Sun's disk, is decreased from the same causes which produce the spurious disk of a star in the focus of a lens. When the true limb of *Venus* falls in a line with the true limb of the Sun, the light is cut off, the spurious enlargement of the Sun's disk and encroachment on that of *Venus* is destroyed at the point of contact, and, as a consequence, a dark ligament appears to connect the apparent limbs of the Sun and *Venus*. This is the real internal contact.

The breadth of the connecting ligament rapidly increases, and, after some 18^s, the exact interval depending upon the circumstances of the transit and the success in first catching phase (1), the limbs of *Venus* and the Sun will appear in contact.

This is the second phase for accurate observation, the apparent internal contact. The observation of this phase is rendered more difficult by the presence of the connecting ligament. The observation, however, is of great value on account of the probable loss of phase (1) at many of the stations through atmospheric disturbance.

It will be seen from this description that the passage from the real to the apparent internal contacts is gradual. The real contacts will appear to take place instantaneously to all the observers. The description of the formation or disappearance of the connecting ligament as "instantaneous" will convey no definite information respecting the phase really observed. The required information can only be conveyed by a drawing representing, as accurately as possible, the relative breadth of the ligament, as compared with the diameter of *Venus*, when last seen at the ingress and first seen at the egress. It may be mentioned that *Venus* will appear quite within the Sun's disk at the disappearance and first appearance of the connecting ligament.

It should also be borne in mind that—if it were possible for any observer, favoured by exceptional atmospheric circumstances,

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to employ extraordinary optical power and thus catch a glimpse of the connecting ligament at ingress much later and at egress much earlier than the average phase, as marked by the breadth of the ligament, caught by the rest of the observers — then such abnormal observations, however interesting for other purposes, would be absolutely injurious for the accurate determination of Solar Parallax. It is, therefore, most desirable that all the observers should employ the same optical power, which must not be fixed too high. A power of about 150 would, perhaps, meet all requirements. I would, therefore, suggest for consideration,—

1. That real internal contacts be observed.
2. That apparent internal contacts be observed.
3. That a magnifying power of about 150 be employed.
4. That all the observations should be accompanied by drawings representing, as accurately as possible, the *relative breadth of the connecting ligament as compared with the diameter of Venus*, corresponding to the time of each observation. At the real internal contact, at ingress, the breadth of the ligament when last seen should be represented.
5. Of course full notes would be given of the circumstances under which the observations were made.

On the Observation of the Transits of Venus by means of Photography. By Warren De La Rue, Esq.

The Astronomer Royal having intimated to me his intention of communicating to the Society a paper on the Transits of *Venus* of 1874 and 1882, and having invited me to make any suggestion which may have occurred to me on the means which Photography offers for their observation, I venture to lay the following statement before the Society: premising, however, that I do not suggest that photographic observations should displace eye observations; on the contrary, I think that both eye and photographic observations ought to be made.

It will be recollected that in 1860 I made photographic observations of the phenomena of totality, and also of the different phases of the eclipse of the Sun before and after that of centrality; the photographs of the totality and of partial phases were measured by means of a micrometer which I designed for that purpose,* and the results tabulated† and discussed.

From the discussion of the measurements of the partial phases, the angular position of the Moon's centre in relation to the Sun's centre and the distances of the Sun and Moon's centres were derived and compared with these elements calculated by Mr. Farley.‡

* This is described and represented in *Phil. Trans.*, 1862, pp. 373–374,
† Ib. p. 383. ‡ Ib. p. 387, Table III. columns.

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On reference to these results, it will be seen that the observed and calculated positions agree remarkably well,* and that the deduced quantity for the nearest approach of the Sun and Moon's centres only differs by $\frac{9}{10}$ths of a second of arc from that calculated by Mr. Farley and 1.2 second from that calculated by Mr. Carrington.

The conditions which transits of *Venus* offer for the determination of the relative position of the Sun and planet's centres are more advantageous than those presented by solar eclipses, inasmuch that it is far more easy to measure directly the distances between the centre of the disk of the Sun and that of the image of the planet upon it, than it is to measure the distances between the peripheries of the Sun and Moon,† or the angular opening of the cusps‡ of the partially eclipsed Sun. And in transits of *Venus* any error of observation would not affect the final result nearly so much as in solar eclipses; for example, in the transits of 1874 and 1882, an error of 1″ in the measurement would, for the maximum displacement, give an error of only 0″·185 in the deduced Solar Parallax.

Moreover, it may be observed that in photographic records it is by no means important to catch exactly the phases of contact, as two photographs obtained at a sufficient interval afford the means of calculating to a great degree of refinement and of tracing the path of the planet, which, for the conditions of the problem, may be considered to be a straight line between the two positions recorded.

Nor is it in any way essential, as it is with eye observations, that favourable conditions should exist for retarding the period of contact at one station and of accelerating it at another, because the chords representing the planet's path can be derived from photographic records, with as much accuracy under what would be considered unfavourable conditions as under favourable conditions for eye observations, for the length of the chords need not be directly considered in determining the nearest approach of the Sun and planet's centres.

During the duration of the transit it would be possible in a clear state of the atmosphere to obtain a series of photographs at intervals of two or three minutes, and any or all of these would be available for comparison with the records obtained at all the stations selected.

The epoch of each photographic record is determinable with the utmost accuracy, 1st, because the time of exposure is not more than the $\frac{1}{50}$ or the $\frac{1}{100}$ of a second, and 2nd, because the instantaneous slide, as it flashes before the secondary lens, affords an audible signal§ by striking against a stop a small fraction of second after it has shut off the image of the Sun. This interval might be determined by experiment and taken into account.

* *Phil. Trans.* 1862, p. 393, Table IV.
† Ib. p. 383, Table I.
‡ Ib. p. 55, Table III.
§ Ib. p. 364.

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In figures 1 and 2 is represented the solar disk of the dimensions of the photographs which would be given by the Kew Photoheliograph at the epochs of 1874 and 1882, under the same conditions of adjustment as existed during the eclipse observations of 1860; on this occasion $\frac{1}{1000}$th of an inch represented 0″·496 second of arc;* the images are direct, as given by that instrument.

The following data, part of which are derived from Mr. Hind's paper in the *Comptes Rendus* for July 22, 1861, on the circumstances of the Transits of *Venus* for 1874 and 1882, have been used in setting out the diagrams.

1874.

⊙ Semi-diameter (Le Verrier)	16′ 14″·98
⊙ Inclination towards East of Axis to a Meridian drawn through Sun's centre	12° 49′
Heliographic Latitude of Earth S.	22′
♀ Semi-diameter	31″·41
Ingress, Ext. Contact 49°·4 N. towards E.	
Egress, Ext. Contact 20° N. towards W.	
♀ Horizontal Parallax	32″·44
⊙ ,, ,, (by Encke)	8″·71

1882.

⊙ Semi-diameter (Le Verrier)	16′ 14″·64
⊙ Inclination towards East of Axis to a Meridian drawn through Sun's centre	13° 52′
Heliographic Latitude of Earth N.	12′
♀ Semi-diameter	31″·40
Ingress, Ext. Contact 145°·3 N. towards East	
Egress, Ext. Contact 113°·9 N. towards West	
♀ Horizontal Parallax	32″·43
⊙ ,, ,, (by Encke)	8″·71

In the Kew Photoheliograph the solar disk would, at the epoch of the transit of 1874, have a semi-diameter of 1965·8 thousandths of an inch (a diameter of nearly four inches), *Venus* a semi-diameter of 63·33 of these units, and the parallax of *Venus* referred to the Sun would be represented by 47·85 of these units; the maximum possible displacement being 95·7 units, or nearly $\frac{1}{10}$th of an inch. In 1882 the Sun's semi-diameter would

* *Phil. Trans.* 1862, p. 383, Table I.

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be 1964·9 units; that of *Venus* 63·31 units; the parallax of *Venus* referred to the Sun 47·82 units; the maximum possible displacement 95·6 units.

When the photographs have been secured, the measurements by means of the micrometer, which would have to be performed, consist in determinations of the Sun's semi-diameter, in units of the arbitrary scale of thousandths of an inch, the angle of position of *a*, *b*, *d*, &c., and the distances *a c*, *b c*, *d c*, figs. 1 and 2; with these data the portion of the chord *a b*, *b d*, &c., could be calculated. The measurements by means of the micrometer described in the *Phil. Trans.* 1862, pp. 373-374, can be obtained to the $\frac{1}{2000}$th of an inch (0″·25), and the position angles to one or two seconds of arc. For each photograph measurements made at different times are remarkably accordant; the greatest difference between the semi-diameter of the Sun of the several eclipse pictures of 1860 was $\frac{9}{1000}$ths of an inch, or about 4″·5; but, on taking the mean of measurements of forty-five photographs by two different methods, the difference was only $\frac{1.5}{1000}$, or about 0″·75. I am inclined to believe that the distance *a b*, *a c*, could be ascertained to within 1″, by means of a few pictures, and possibly to 0″·25, if a sufficient number of photographs were obtained.

Since 1860 means have been devised of ascertaining the optical distortion of the photographic pictures, and, in a paper* recently sent in to the Royal Society by myself, Mr. Balfour Stewart, and Mr. Loewy, this has been treated of; if this optical distortion had been ascertained for the photographs of the eclipse, 1860,† it is certain that even more accurate results would have been obtained, because a part of the small discrepancies in the measurements arose out of the circumstance of the centre of some of the pictures not being in the optical axis of the instrument. In the eclipse pictures the whole disk of the Sun was not, however, visible, but in the transit pictures the whole disks of the Sun and *Venus* would be seen, and the determination of the requisite data, consequently, far more accurate. By photographing a suitable scale of equal parts placed at about a mile or two miles from the heliograph, a picture would be obtained in which different equal portions of the scale would occupy different lengths; by measuring the divisions by means of the micrometer, the effect of optical distortion could be ascertained to a great nicety.

Such a scale of equal parts, if erected at a distance of two miles, would have to be about 110 feet long, in order that its image might be somewhat longer than that of the diameter of the Sun picture. It could be made by fixing two horizontal rails of

* Now being printed in the *Phil. Trans.*

† It is not possible to do so now on account of the relative adjustment of the object-glass and secondary lens having been disturbed after the observations had been made.

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wood, one over the other, on vertical posts, so that their outer edges would be distant about 3 feet. On these horizontal rails plates of zinc, 2 feet wide and 3 feet long, would have to be fastened so as to leave an interval of exactly 2 feet between two adjacent plates. These plates could all be made of exactly the same width by clamping the whole series together and planing their edges in a planing machine; in fixing them on the horizontal rails, one of the plates could be used temporarily as a template to govern the interval between a plate previously screwed down and one about to be fixed. The horizontal bars would be strengthened here and there by cross bars of wood behind the plates and by cross tension rods properly distributed. To facilitate the depiction of the scale, it should be placed on the summit of a low hill or, by preference, near the sea-shore, in order that the light of the sky might shine through the intervals between the plates.

The focus of the object-glass of the Kew Heliograph, for an object situated at a distance of two miles, would be about $\frac{8}{100}$ths of an inch longer than that for parallel rays; this elongation of the focus produces a little indistinctness in the pictures, but does not prevent measurements of them being made. Before finally fixing the position of the secondary (or enlarging) lens for Sun pictures, sharply defined pictures of the scale could be procured by adjusting the focus accurately for the scale of equal parts; subsequently other pictures of the scale could be procured after the focus had been adjusted and fixed for Sun pictures. This double set of pictures would afford all the requisite data for ascertaining the correction for optical distortion; and, the exact distance of the scale of equal parts being known, for obtaining the value in arc for the divisions of the micrometer.

Fears have been expressed that the collodion, in drying, becomes distorted; experiments, however, in 1860–61 have demonstrated that the shrinkage is only in the direction of the thickness.* But, as in the case of the Solar Parallax no refinement of correction ought to be neglected; it would be quite easy to ascertain once more whether any distortion does take place, by taking photographs on glass plates on which lines about $\frac{1}{4}$ of an inch distant had been previously etched, the collodion, which should be rendered purposely contaminated with particles in suspension, should be poured on the ruled sides to avoid parallax:—after all the operations of photography the film would have to be examined from the back, and the position of certain impurities with reference to the ruled lines noted whilst the collodion was wet, and after it had dried.

Lastly, on looking at the charts given in the May number of 1857, and in the June number of 1864, in Mr. Airy's valuable papers, it appears that, at all events, six stations could be selected favourable for photographic records: I would propose that six

* *Phil. Trans.* 1862, p. 36.

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precisely similar instruments should be prepared and mounted equatoreally, but without circles, or a driving clock, as these are quite unnecessary. The optical distortion of these instruments could be determined beforehand, and no further experiment would be necessary, as all the optical parts must be rigidly fixed; it would only be necessary to make a provision for the rotation of the telescope in a cradle on the declination axis for the object of adjusting the cross wires in a position of 45° to a meridian passing through the Sun's centre; in other respects the mounting would be of the simplest kind.

No difficulties exist in photographing a transit of *Venus;* the operations are quite the same as those practised daily at the Kew Observatory; no strain on the nerves would occur, as in the anxiety consequent on the desire of rendering available every moment of the short duration of a solar eclipse. All the operations could be conducted with that calm so essential for such a problem as the determination of the Solar Parallax, and I feel confidence in recommending that timely steps should be taken to secure photographic records of the Transits of *Venus* in 1874 and 1882.

Ephemeris for Observations of the Physical Features of the Planet Mars during the Opposition of 1869. By A. Marth, Esq.

(*Communicated by R. S. Newall, Esq.*)

The following Ephemeris may probably be of service to those observers who are engaged upon observations of *Mars.* It is computed on the assumption that the axis of rotation of the planet is directed to the point R.A. 317° 51′, P.D. 39° 46′, and that the time of rotation is = $24^h\ 37^m\ 22^s{\cdot}62$ mean time. The Ephemeris gives for 8^h Greenwich Sidereal Time of every 5th day the values of—

L, the Areographic Longitude of the Apparent Centre of the Planet's Disk, reckoned from an arbitrary First Meridian to the West.

B, the Aerographic Latitude of the Centre, or the Elevation of the Earth above the plane of Mars' Equator.

P, the Angle of Position of the visible Pole at the Centre.

Q, ,, ,, ,, greatest Phase.

q, the Breadth of the greatest Phase } the Diameter of Mars at the Distance
d, the Diameter of the Disk } 1 being assumed = 9″·328.

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8h Gr. Sid. T. 1869.	L.		B.	P.	Q.	q.	d.
	°	°	° ′	° ′	° ′	″	″
Jan. 1	260·0	−1·1	24 21 N.	13 56	289 4	0·66	10·89
6	209·7	−0·7	24 11	13 57	288 24	·59	11·36
11	159·8	−0·4	23 57	13 46	287 29	·50	11·83
16	110·2	0·0	23 41	13 22	286 14	·40	12·29
21	61·0	+0·3	23 22	12 44	284 26	·30	12·72
26	12·1	+0·5	23 2	11 53	281 47	·20	13·10
31	323·4	+0·8	22 40	10 51	277 23	·12	13·41
Feb. 5	275·0	+0·9	22 16	9 40	268 21	·05	13·64
10	226·7	+1·0	21 52	8 22	240 59	·01	13·76
15	178·5	+0·9	21 27	7 1	161 49	·01	13·76
20	130·2	+0·7	21 4	5 40	132 55	·05	13·66
25	81·7	+0·5	20 44	4 24	122 54	·11	13·44
Mar. 2	33·0	+0·3	20 26	3 17	118 5	·19	13·14
7	344·1	−0·1	20 14	2 20	115 9	·29	12 77
12	294·8	−0·4	20 7	1 34	113 10	·39	12·35
17	245·2	−0·7	20 6	1 2	111 48	·48	11·90
22	195·3	−1·1	20 11	0 44	110 45	·58	11·44
27	145·0	−1·4	20 21	0 39	110 1	·65	10·97
Apr. 1	94·4		20 37 N.	0 45	109 32	0·71	10·52

Increase of L for other Hours and Days.

Gr. Sid. T.	0	1	2	3	4
h	°	°	°	°	°
4	301·6	291·8	282·0	272·1	262·3
5	316·2	306·4	296·6	286·7	276·9
6	330·8	321·0	311·1	301·3	291·5
7	345·4	335·6	325·7	315·9	306·1
8	0·0	350·2	340·3	330·5	320·6
9	14·6	4·8	354·9	345·1	335·2
10	29·2	19·3	9·5	359·7	349·8
11	43·8	33·9	24·1	14·3	4·4
12	58·4	48·5	38·7	28·8	19·0
13	73·0	63·1	53·3	43·4	33·6
14	87·5	77·7	67·9	58·0	48·2
15	102·1	92·3	82·5	72·6	62·8
16	116·7	106·9	97·0	87·2	77·4

Hourly Increase, 14°·59.

This is the increase for the average difference of the successive values of L, which is = 1750°·80. The deviations from this average, which depend on the variation of the planet's apparent

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motion and distance, are given above as differences between the values of L, and must be allowed for where needed.

To diminish the existing uncertainty of the direction of the axis of rotation, trustworthy measures of the angles of position of some well-defined spot near the pole with respect to the centre of the disk are required. The spring equinox for the northern hemisphere of *Mars* has occurred, 1868, Sept. 28, and the summer solution will take place, 1869, April 14. It seems, therefore, not unlikely that, during the next months, the white spot (supposed to be snow) near the pole will offer a good object for observation. But, to get rid of the assumption that its centre occupies a constant position on the ball of the planet, and of the still greater assumption that it coincides with the pole itself, it is necessary that trustworthy angles of position should not only be observed on a sufficient number of nights, but also that, if practicable, two or three sets of observations be taken each night at intervals of some hours.

Though liable to some uncertainty, the direction of the axis and the time of rotation are sufficiently well known for the purpose of placing the drawings of *Mars*, which have been made during the last ten or twenty years, in their proper order. A list of the sketches known to us, arranged in the order of the areographical longitudes and latitudes of their centres, will be communicated when it can be given with some completeness. At present, probably several series of published sketches are not yet at our disposal. Of unpublished drawings there are very likely some series which may deserve to be at least consulted. Several of them are probably in the library of the Royal Astronomical Society. Observers who desire to have their sketches inserted in the proposed list will, perhaps, consider it worth while to communicate to us (at Ferndene, Gateshead), if not copies or tracings, at least the approximate times (if possible within a few minutes) to which their drawings refer.

A statement of the values of B and P for the times of the last half-dozen oppositions may, perhaps, be of interest to some readers.

		B.	P.
		° ′	° ′
Opposition 1856	April 2	25 49 N.	33 35
58	May 15	12 4 N.	40 43
60	July 17	14 22 S.	189 26
62	Oct. 5	22 40 S.	145 17
64	Nov. 30	6 25 S.	142 40
67	Jan. 10	10 38 N.	343 54

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Description of Drawings of Mars. By J. Joynson, Esq.

The accompanying sheets are lithographed copies of a series of diagrams already sent to the Society. They show in consecutive order the appearances presented by the planet during one complete rotation, at each opposition, from 1862 to 1867. They have been lithographed for the purpose of sending a copy of each to any Member of the Society or others interested who may apply for them, and at the same time undertake to send me a similar series, to be taken by the applicant during the coming opposition; as, by this means, it is hoped that something near a correct delineation of the planet during one complete rotation will be obtained. It is earnestly requested that, as far as possible, every observer will obtain the appearances during the whole rotation, and so avoid the mistake that has been made of supposing that the same appearance was presented by two different parts of the planet. It is also particularly desired that no approach to fancy sketches may be made, or any attempt permitted in the way of striking drawings; but that they be strictly confined to the appearance presented by the planet at the time noted at the foot of each drawing. The aperture of the telescope and the power used should be given.

The plan adopted by the writer was to commence the series as soon as the planet presented well-defined markings on the disk, and then make a drawing about every forty minutes. By this means four or five drawings were made; and by doing the same on the next evening, commencing about the same time, the observer would soon see that the drawings arranged themselves somewhat in this order;—

					st	nd	rd	th	th
1st Night's Drawings					1	2	3	4	5
2nd	,,			1	2	3	4	5	
3rd	,,		1	2	3	4	5		
4th	,,	1	2	3	4	5			

That is, he would get about one aspect sooner each night. What is seen, say at 7 o'clock on the first evening, is not seen on the next until $7^h\ 37^m$, and on the following until $8^h\ 15^m$, varying about 37 minutes each succeeding night.

However the drawings are originally made, — that is, with reference to the Vertex at the Pole of the Earth,— it will be desirable to place them all in the final drawing with the pole of the planet in one direction.

Waterloo, near Liverpool, October 2, 1868.

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The Transit of Mercury. By John Browning, F.R.A.S.

On the morning of November 5th the Sun rose to view at my small telescope about $8^h\,15^m$. At this time *Mercury* was at once seen as a very dark purplish-grey spot on the solar disk. The limb of the planet was ill defined, perceptibly darkest towards the centre, and surrounded by an aureola slightly brighter than the solar disk. Near the centre of the planet appeared two bright grey spots close together.

The spot to the N.W. seemed to be much the most conspicuous, and to be elongated, in the direction of the smaller spot. Possibly the faint spot was produced by the boiling of the image, which was sometimes very bad.

Mr. Pritchard has kindly informed me that the shadow of a black disk is a white spot. Possibly this may prove the explanation of these otherwise unaccountable appearances.

When the limb of the planet was still distant nearly one-third of its diameter from the Sun's limb, the limb of the planet seemed drawn out towards the Sun's limb. About 23^s after this the contact seemed complete.

For this observation I used a silvered-glass reflector of $4\frac{1}{2}$-in. diameter, and $5\frac{1}{2}$ feet focus, made for me by Mr. With. The mirror had been tested up to 700 on stars. The Sun being low I did not use a solar eye-piece, but simply modified the light with a sliding wedge of dark neutral tint-glass corrected for refraction.

Of the three drawings I have the honour of exhibiting before the Society the first represents the planet and shows the aureola and the two white spots. The other two show the appearance of the planet at the times of real and apparent contact with the Sun's limb.

As I did not see either the aureola or the white spots towards the time of contact, I have omitted them in the figures which represent the contact.

The Transit of Mercury. By C. G. Talmage, Esq.

The transit of *Mercury* was well seen here. At sunrise the sky was perfectly cloudless, and remained so until nearly noon. When I first directed the telescope to the Sun at $19^h\,50^m$ M.T. the limbs were boiling violently, but by $20^h\,30^m$ the definition was greatly improved. From the first the limbs of *Mercury* were much better defined than those of the Sun. I watched very carefully for any apparent distortion of the limbs of *Mercury*, but saw none. The time of internal first contact I consider better than the time of last contact.

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	Egress.	
	h m	h m
First contact	21 0	12 38
Last contact	21 3	5 70

I used the full aperture of 10 inches, with a diagonal eye-piece. Power 200.

Mr. J. G. Barclay's Observatory,
Leyton, Essex.

The Transit of Mercury. By G. Knott, Esq.

Although the morning of November 5th was fine, definition in the telescope was so disturbed that the transit of *Mercury* was not well observed at this station. With an aperture of 2¾-in. and a negative eye-piece magnifying 89 times, I observed the egress as follows,—

	h m s	
First contact, Nov. 4	21 0 25·9	G.M.T.
Last contact	21 2 32·6	

both results being very uncertain. With the full aperture, 7¼-in. and a Dawes' solar eye-piece with powers 95 and 145, I carefully scrutinised the planet during intervals of tolerable definition. At times I fancied that I saw traces of a narrow penumbra surrounding the body of the planet, but when best seen the disk appeared to be uniformly black, and the bright spot described by some observers, though looked for, was not seen. In a transient interval of pretty steady definition some few seconds before first contact, I caught a glimpse of a fine black ligature connecting the planet with the Sun's limb. The "boiling" of the limb was so troublesome that the observed times of contact may be some seconds in error.

Woodcroft Observatory, Cuckfield,
December 10, 1868.

Description of a new Chronograph Pen. By R. L. J. Ellery, Esq., Director of the Observatory of Melbourne.

The drawing, which gives the full size of the pen, is in the woodcut reduced to the scale of one-third.

Fig. 1. *a* is the reservoir of glass, on which a brass cover, *b*, *b'*, is cemented. The syphon, *e*, *e*, *e'*, is made of spirit thermometer tube, about 0·2 inch diameter, with a bore of 0·05 inch, and is bent as shown, so that one leg reaches nearly to the bottom of the reservoir; it is fastened to the cover by a joint like a gas-union, one part of which is cemented to the tube, another part is soldered into the cover, and the clamp screw M draws the two parts together air-tight; the length of the tube will depend on the size of the barrel, distance of electro-magnet, &c. At *e'* the tube is bent at a convenient angle, and the end ground square,

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on this end a tube carrying a screw-thread is cemented, into which the nib is clamped as shown at *n n*. To support the tube an arm, *h h*, of thin brass projects from the reservoir cover, and ends in a notch at *o*, in which the tube lies, and in which it is firmly retained by means of the collar and the screw, *f*, *g*. The air tube, *c d*, is a piece of tube like the syphon, but reaching to

Fig 1

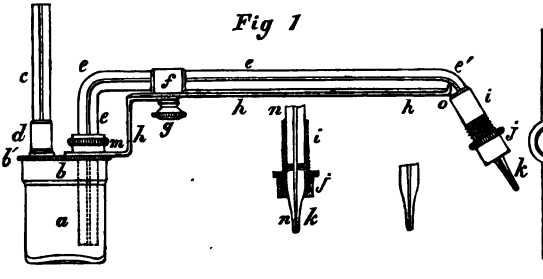
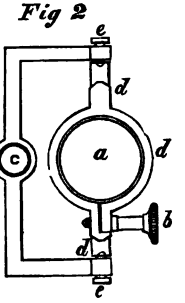


Fig 2



just within the cover. The cover *b'* itself unscrews from the collar *b*, which is permanently cemented to the reservoir. The nib is made of same sized tube as the syphon by drawing out in a blow-pipe flame ; some skill, however, is required to reduce the bore gradually towards the point, and at the same time retain sufficient glass around it. The nibs are ground square at the thick end, and cut off to the required length or to the required fineness of bore, and then ground to a rounded point on a stone or with fine emery, and slightly polished on Arkansas stone or with rouge to take away any asperities likely to catch the fibres of paper. Sketch *n n* shows how it is fitted on ; a perforated washer of sheet India rubber being placed between the ends of the tube and nib. The fluid used is a strong solution of Judson's solid aniline blue in boiling distilled water. To fill the pen the cover is unscrewed, or *d* taken out, and the requisite quantity of fluid put in; screwing the cover on air-tight and blowing into the air-tube C forces the ink through the syphon into the nib, from whence it oozes in a drop. The drop being removed and the pen adjusted in a suitable holder on the armature lever of the electro magnet, with the nib resting on the revolving barrel, the ink will flow as freely or as slowly as required, by altering the "head," which is readily done by lowering or raising the reservoir in the clip or holder, of course the pressure of the pen on the barrel or paper must be regulated by suitable springs or counterpoises.

The advantages of this pen are that any sediment that may occur in the fluid falls to the bottom of the reservoir, and is never likely to reach the fine bore of the nib. When once filled it can be used day after day till empty; the flow, if stopped by drying at the nib, can be usually re-established by rubbing with the moistened finger, or, at the worst, by removal of the nib. The flow can be regulated by raising or lowering the reservoir in its

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clip, and very fine or very strong lines obtained by using nibs with finer or coarser bores.

Fig. 2 represents the arrangement by which the pen is attached to the electro-magnet armature. C is the armature axis; *d d d*, the pen-holder suspended between two centre screws *e e*.

The reservoir is clamped in the ring *a*, which is lined with a piece of leather and clamped by screw *b*.

Observations of the Meteoric Shower of November 13–14, 1868, *made at the Glasgow Observatory.* By Prof. Grant.

During the earlier part of the night the sky was so overspread with dense vapours that nothing could be seen. About $15^h\ 30^m$ the clouds commenced slowly to disperse, and through the openings between them a meteor might now and then be seen shooting across the heavens. The state of the sky continued to improve till about $16^h\ 30^m$, when it finally became pretty clear in all directions. It now became evident that a shower of meteors was in progress; nor did it require long time to be convinced that the phenomenon was no other than an apparition of the great November shower which had been so conspicuous during the two or three previous years. In every instance the course of the meteors was found, when traced back, to emanate from the constellation *Leo*, the radiant appearing to coincide very nearly with the middle point of a line joining γ and ϵ of that constellation. At $16^h\ 20^m$ the number of meteors visible amounted to two or three in a minute. In their general features they presented no essential difference from the meteors observed on the occasion of the memorable apparition of November 13–14, 1866. Their colour was white, mingled in some instance with a slight tinge of red. They generally were accompanied by trains which lasted two or three seconds. I failed, however, to discover any trace of the beautiful green which formed so interesting a feature of many of the meteors of November 1866. So long as they were attached to the meteors the trains appeared white, but on breaking up they assumed a greyish aspect slightly tinged with red. In three or four instances I remarked that the *débris* of the train continued visible in the heavens for two or three minutes after the disappearance of the meteor. Some of the meteors were of considerable size, three or four of them far exceeding *Jupiter* in brightness, but not equalling the planet *Venus*, which was shining with intense brilliancy in the east, and formed an excellent standard of comparison for estimating the brightness of the larger meteors. It was interesting to note the excessively short trains of the meteors in the immediate vicinity of the radiant point. I failed, however, to perceive any instance of a meteor which appeared absolutely motionless during the time of its visibility.

The shower continued sensibly to increase from $16^h\ 30^m$ till $16^h\ 56^m$, and as it appeared very desirable to endeavour to ascer-

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tain the time of its maximum, I now proceeded, in conjunction with Mr. John McKinnel, the junior assistant, to count the number of meteors which might become subsequently visible. The results are given underneath. It would appear from them that the maximum of the shower occurred at $17^h\ 15^m$.

Shortly after 18^h it became cloudy, and we ceased to obtain any further observations of the phenomenon, but although the greatest intensity of the shower seemed to have already passed, it was very evident that it had not yet wholly expended itself.

Time. G.M.T.				Number of Meteors Counted.
h	m	h	m	
16	56 to	16	57	1
16	57	16	58	2
16	58	16	59	6
16	59	17	0	8
17	0	17	1	5
17	1	17	2	5
17	2	17	3	6
17	3	17	4	2
17	4	17	5	7
17	5	17	6	8
17	6	17	7	7
17	7	17	8	5
17	8	17	9	2
17	9	17	10	4
17	10	17	11	9
17	11	17	12	6
17	12	17	13	11
17	13	17	14	4
17	14	17	15	3
17	15	17	16	3
17	16	17	17	3
17	17	17	18	5
17	18	17	19	2
17	19	17	20	1
17	20	17	21	5
17	21	17	22	4
17	22	17	23	6
17	23	17	24	4
17	24	17	25	3
17	25	17	26	3
17	26	17	27	7
17	27	17	28	3
17	28	17	29	5
17	29	17	30	3
17	30	17	31	3

Time. G.T.M.				Number of Meteors Counted.
h	m	h	m	
17	31 to	17	32	2
17	32	17	33	3
17	33	17	32	4
17	34	17	35	5
17	35	17	36	4
17	36	17	37	1
17	37	17	38	0
17	38	17	39	4
17	39	17	40	4
17	40	17	41	3
17	41	17	42	4
17	42	17	43	5
17	43	17	44	3
17	44	17	45	1
17	45	17	46	1
17	46	17	47	2
17	47	17	48	3
17	48	17	49	1
17	49	17	50	3
17	50	17	51	2
17	51	17	52	2
17	52	17	53	2
17	53	17	54	2
17	54	17	55	3
17	55	17	56	4
17	56	17	57	5
17	57	17	58	0
17	58	17	59	1
17	59	18	0	0
18	0	18	1	2
18	1	18	2	6
18	2	18	3	4
18	3	18	4	3
18	4	18	5	2
18	5	18	6	4

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It appears from these results that 256 meteors were counted during the interval of one hour and ten minutes included between $16^h\ 56^m$ and $18^h\ 6^m$. There can be no doubt, however, that the number really visible was considerably greater than this. It was certainly twice, and in all probability was three times as great. We may, therefore, infer that during the interval in question as many as seven or eight hundred meteors were visible.

In order to obtain a clearer view of the time of maximum I have arranged in groups of ten minutes the interval during which the meteors were counted, and have inserted the number of meteors counted in each of the subdivided intervals thus:—

			Meteors Counted.
From	16 h 56 m	to 17 h 6 m	... 50
	17 6	17 16	... 54
	17 16	17 26	... 36
	17 26	17 36	... 39
	17 36	17 46	... 26
	17 46	17 56	... 24
	17 56	18 6	... 27

It would appear from a comparison of these results and the general observations of the phenomenon with the corresponding observations of 1866 that the shower was much inferior in density to that of the great November shower of 1866. We have reason, however, to believe that it was characterised by a more uniform structure.

The Observatory,
Glasgow, Dec. 10, 1868.

Meteors of November 13–14, 1868. By Prof. D. Kirkwood.

The shower of November meteors has been this year unexpectedly brilliant. Unfortunately I was unable to watch the display; but during three hours on the morning of the 13th, Prof. T. A. Wylie, D.D., observed 165. A considerable proportion of these were unconformable. The number, however, radiating from *Leo* was sufficient to indicate the commencement of the shower. Remembering that Humboldt, in 1799, had seen a number of meteors after sunrise, I took a position at 8 o'clock A.M., on the west side of a building (in the shade) and watched the vicinity of the Radiant, in the hope of observing some of the largest meteors. Glimpses of five or six were, as I thought, actually obtained, when, in order to guard against optical deception, I called Mr. Allison Maxwell, a tutor in the State University, who watched about fifteen minutes. This gentleman re-

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ported that he saw *one* beyond doubt, and three others less certainly. I mention the case as being the first, since the commencement of the present century, in which meteors of the November stream have been (probably) seen in the daytime.

On the night of the 13th, a committee of the senior class in the University kept watch from 11 o'clock P.M., till 4.15 A.M., when, having counted 2500, they adjourned. The maximum was about 3.30; 900 meteors having been counted during the 45 minutes immediately preceding. Many of the meteors were very brilliant, leaving long trains, some of which continued visible several minutes. Three or four were observed to explode, or separate into several fragments. No sound, however, was in any case perceived.

At $4^h 55^m$ (40 minutes after the committee at the University adjourned) Prof. Wylie commenced observing at his residence, and continued his watch till $6^h 11^m$, counting 780 in $1^h 16^m$. The whole number therefore actually counted in $6^h 31^m$ was 3280. A portion of the heavens was at times overcast with clouds; especially was this the case shortly after 4 o'clock, when the committee of students adjourned.

At frequent intervals throughout the night a lull occurred in the display; while at other times for a few seconds the meteors were so numerous that they could scarcely be counted.

A remarkable feature in the shower of the present year is its duration. As seen in Europe in 1866, and in this country in 1867, the display was limited to three or four hours. On the present occasion, however, it commenced on the night of the 12th, and had not ceased at daylight on the morning of the 14th. This would indicate considerable irregularity in the thickness of the stream.

Bloomington, Indiana, Nov. 14, 1868.

The Lunar Crater Linné. By W. R. Birt, Esq.

Having been favoured by Mr. Huggins, Capt. Noble, and Herr Von Mädler, with drawings of *Linné* as it appeared on June 26th, Aug. 26th, and Oct. 22nd, 1868, which in many respects perfectly agree with views which I obtained on the 26th of June with the Crossley Equatorial of 7·3-inch aperture, and on the 22nd of Oct. with Mr. Barclay's 10-inch refractor, I have endeavoured from them to construct an approximate section of *Linné* as it existed on Aug. 26. The drawing of this date was made by Tacchini of Palermo, and kindly forwarded to me by Herr Von Mädler. The attempt to construct a section of *Linné* must be considered as exceedingly imperfect, nevertheless it may be of assistance in contributing to more exact observation and measurement at critical periods in the appearance of the crater.

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Two facts appear to have been well established lately, viz., the conical form of the object with its shadow, and the early disappearance of the cone and shadow coincident with the appearance of the white spot. The shortness of the shadow in Capt. Noble's drawing, combined with the Sun's altitude at *Linné*, indicates, I apprehend, that the cone has a very slight elevation above the surface. Assuming both drawings to be moderately accurate as to the proportion of western slope and length of shadow, a simple calculation shows that, taking the length of shadow in Mr. Huggins' drawing = 1·000, the height of the highest part of the cone was on the 26th of June, 0·014, Sun's altitude = 0°46′·5. By measurement *on the drawings* the shadows were as follows, Huggins ·21, Noble ·17 parts of $\frac{1}{2}$ an inch. Tacchini's drawings do not exhibit exterior shadows. Noble's when reduced in the proportion of the drawings = ·12. Now, assuming *no* error in the drawings, nor alteration in the height of the cone, we ought to have had on the 22nd of October with a solar altitude of 1° 7′·69226 as the length of the shadow, that of June 26th being equal to 1·00000. Upon comparing the shadows (reduced to the same scale) that on October 22nd falls short of what it should have been by ·10199, or slightly more than a tenth part. At present it is quite out of our power to decide upon this difference as resulting from errors in the drawings, or measurements from them, or from change in the height of the crater, but it indicates the course we should pursue, viz. to observe the times of disappearance and reappearance of the shadow, also those of the appearance and disappearance of the white spot.

The combination of the drawings by Messrs. Huggins, Noble, and Tacchini, appears to elucidate in some degree the phenomena of the white spot. As soon as the Sun attains an elevation above the horizon equal to the inclination of the eastern slope of the cone, the shadow disappears. This was observed on June 26th, but no observation of the disappearance of the shadow on October 22nd has come to hand. We cannot, therefore, construct a section for October 22nd. Taking in the drawing of Mr. Huggins the length of the shadow = 1·00000, and the Sun's altitude at disappearance of shadow = 1° ± we can find the extent of that part of the base of the cone from the perpendicular passing through the west peak of the small crater to the extremity of the east slope = ·77497. For the determination of the inclination of the west slope we need the altitude of the Sun at the first appearance of the shadow to the west. Most observers agree that the small crater is west of the centre of the white spot. It is therefore probable that the west slope has a greater inclination than the east, and constructing the section to exhibit this we have ·77497 from the west peak to the east base, + ·36000 from the west peak to the west base, = 1·13497 for the approximate diameter of the base. This cannot be received as *accurate* from the absence of data having reference to the west slope. Tacchini says, that according to his measures the diameter of the white spot, including the declivity, was in

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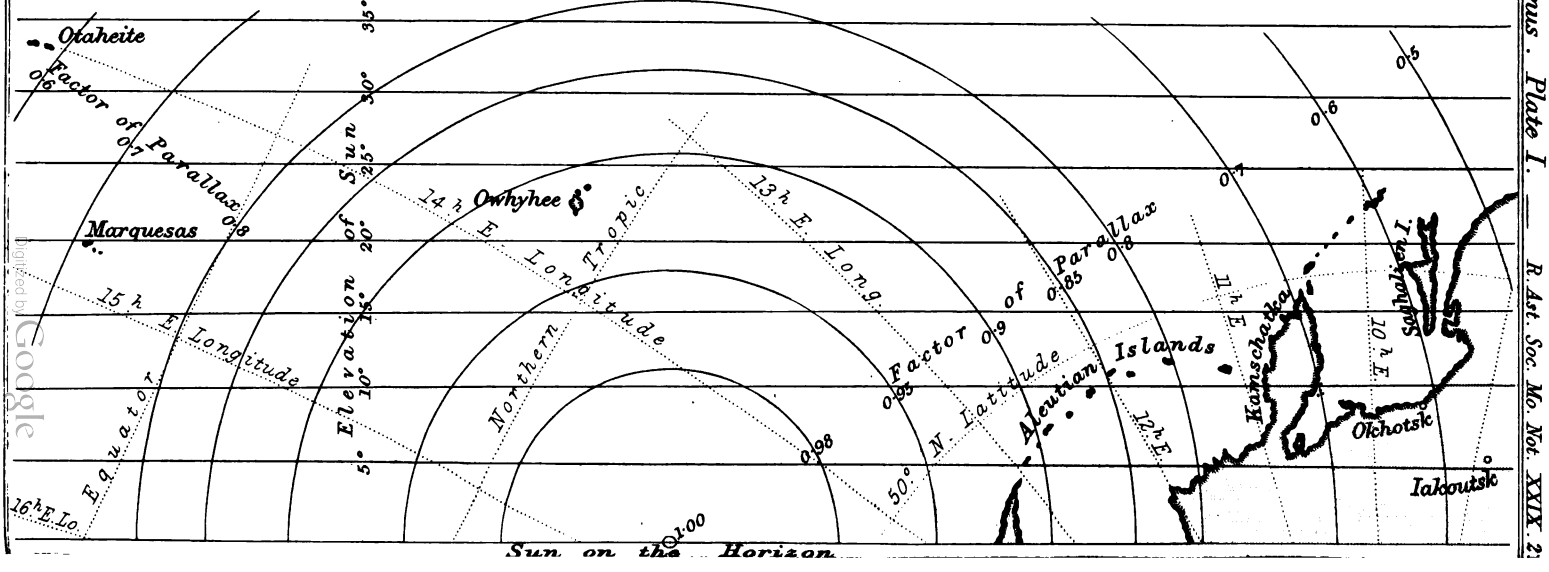
Transit of Venus, 1874, Dec.r 8.

Stations for observation of the Ingress of Venus.

The Sun descending. Venus entering on the Upper Limb.

Ingress accelerated by Parallax.



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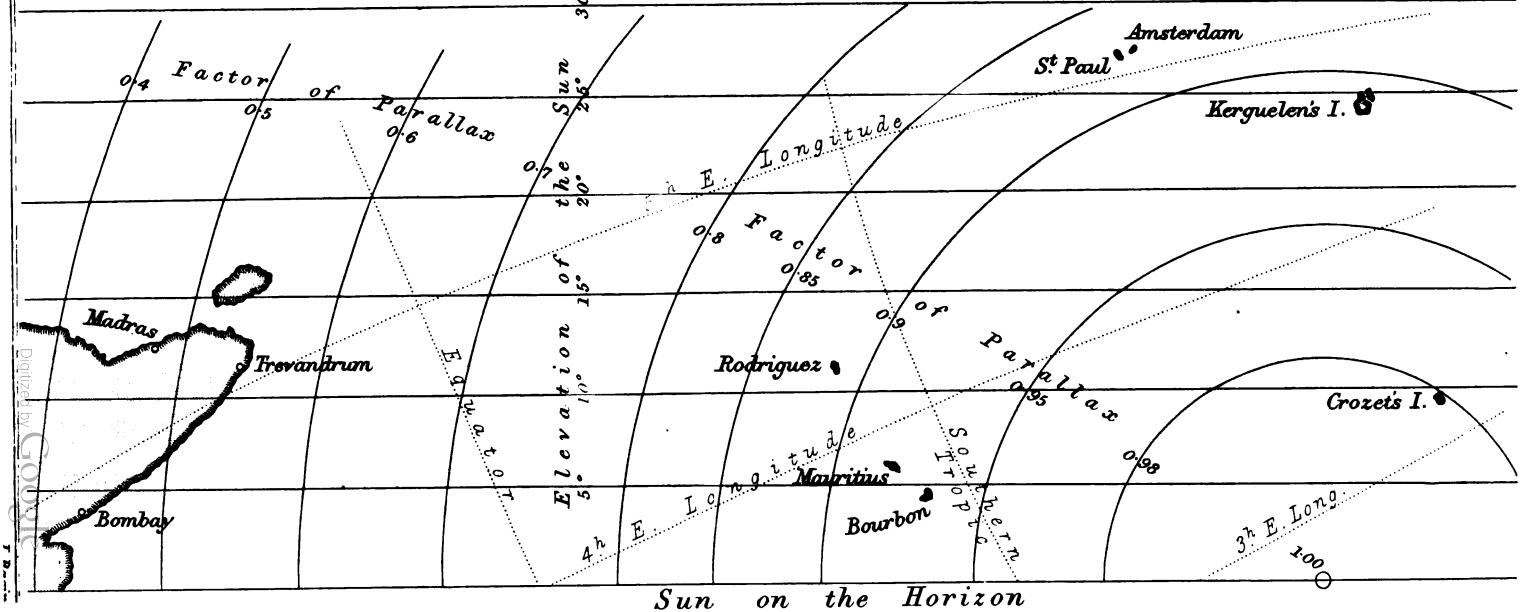
Transit of Venus, 1874, Dec.r 8.

Stations for observation of the Ingress of Venus.

The Sun ascending. Venus entering on the Lower Limb.

Ingress retarded by Parallax.



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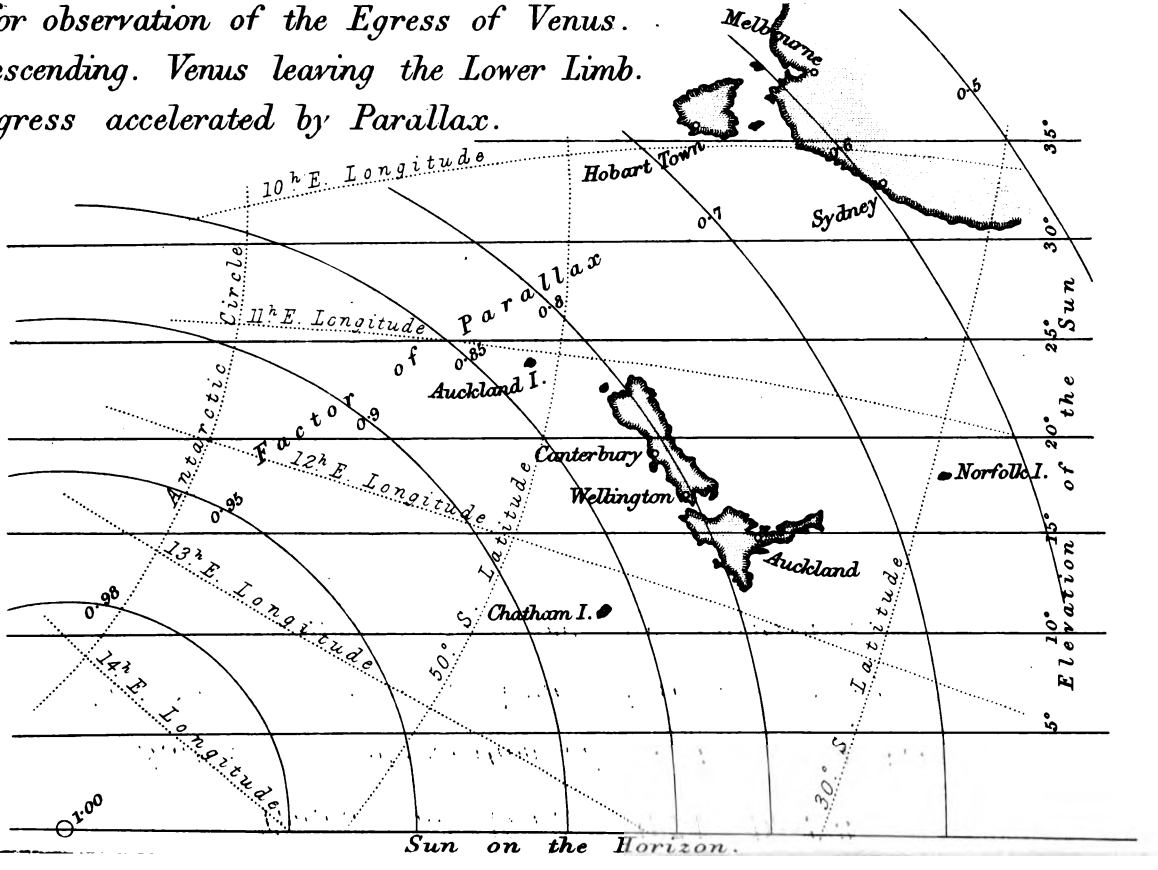
The Sun descending. Venus leaving the Lower Limb.
Egress accelerated by Parallax.



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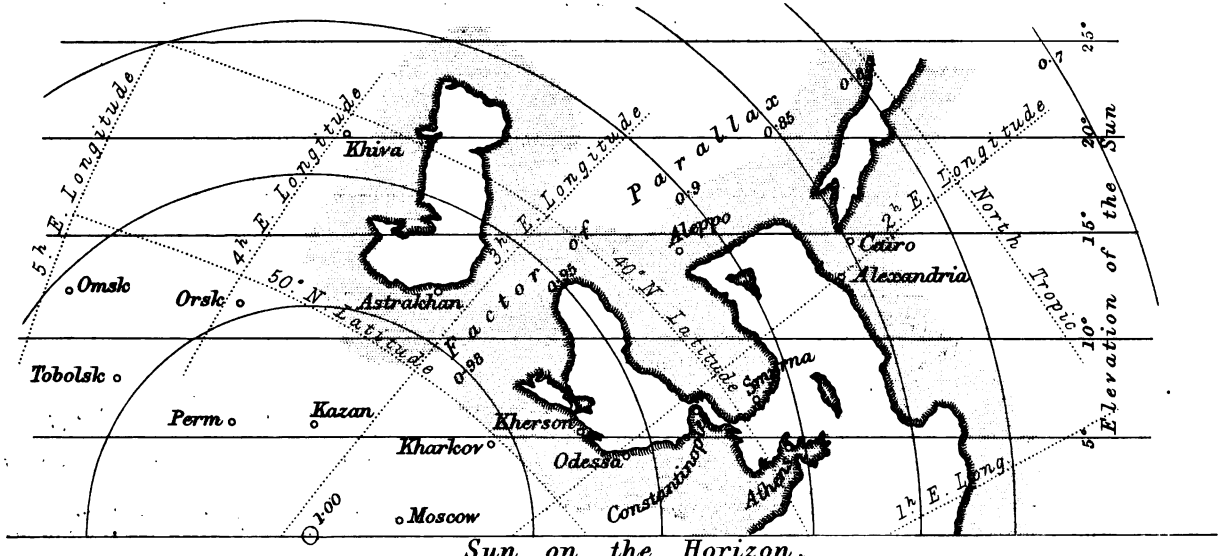
Transit of Venus, 1874, Dec.r 8.

Stations for observation of the Egress of Venus.

The Sun ascending. Venus leaving the Upper Limb.

Egress retarded by Parallax.



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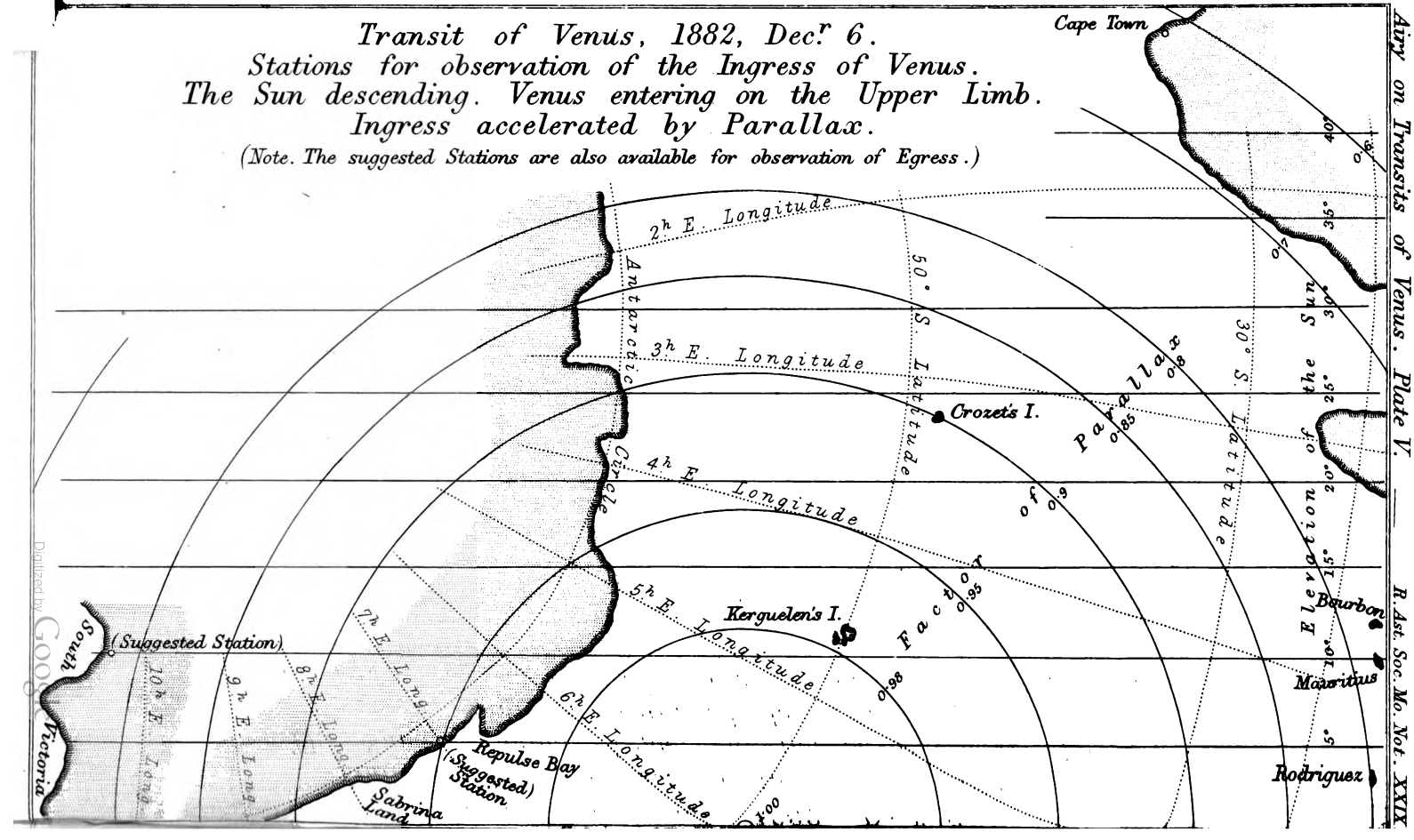
Transit of Venus, 1882, Dec.r 6.

Stations for observation of the Ingress of Venus.

The Sun descending. Venus entering on the Upper Limb.

Ingress accelerated by Parallax.

(Note. The suggested Stations are also available for observation of Egress.)



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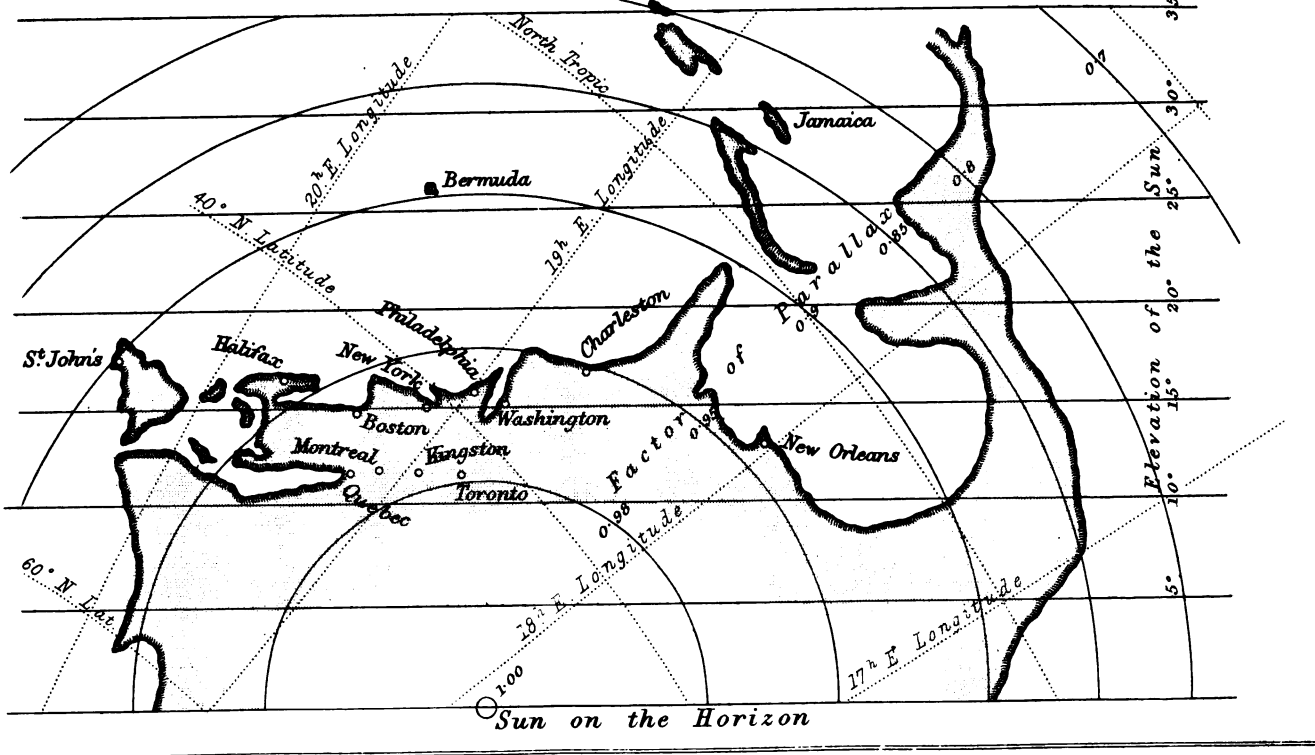
Transit of Venus, 1882, Dec.r 6.

Stations for observation of the Ingress of Venus.

The Sun ascending. Venus entering on the Lower Limb.

Ingress retarded by Parallax.

(Note. The American Stations (except Newfoundland), and the Islands, are also available for observation of Egress.)



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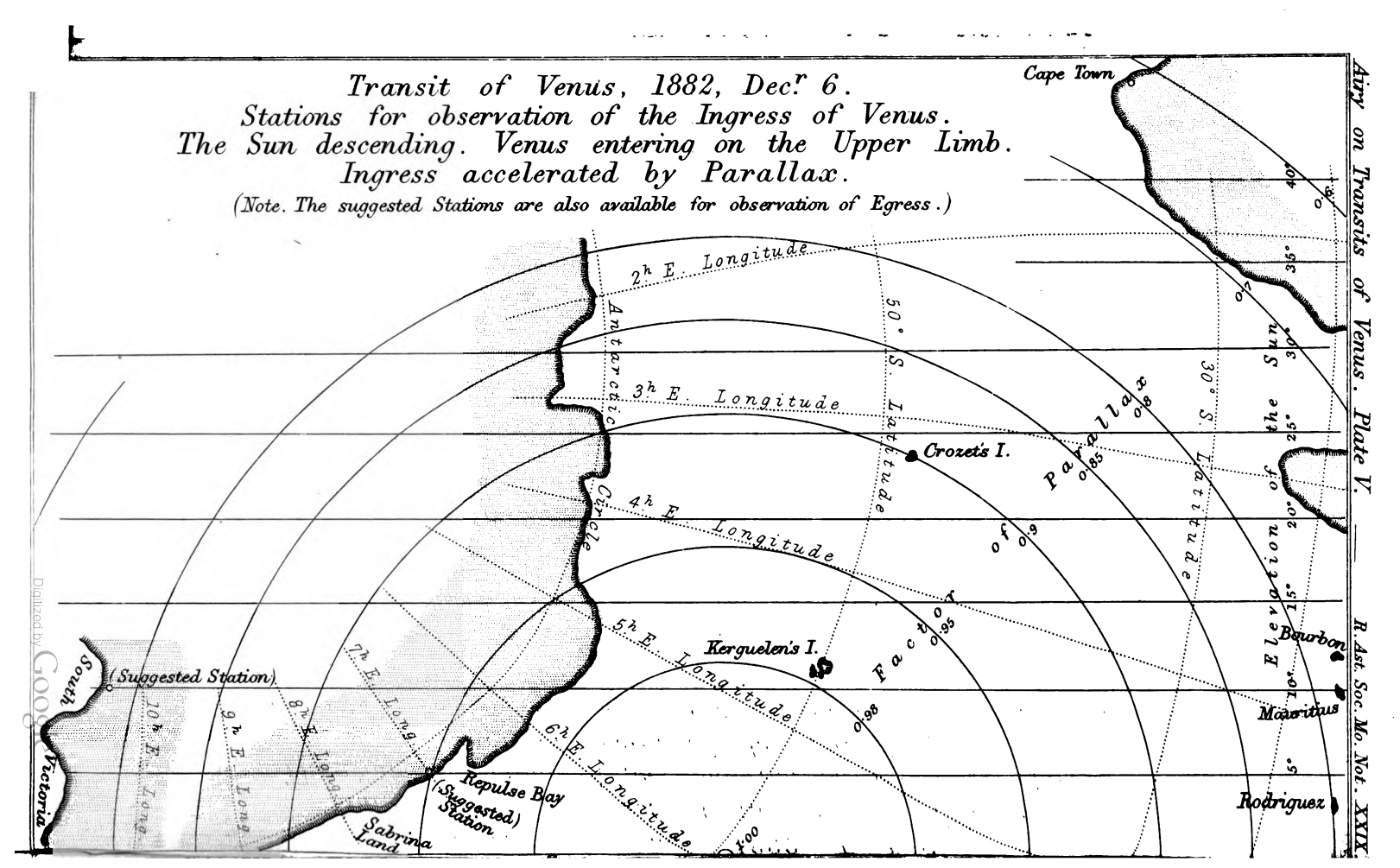
Transit of Venus, 1882, Dec.r 6.

Stations for observation of the Ingress of Venus.

The Sun descending. Venus entering on the Upper Limb.

Ingress accelerated by Parallax.

(Note. The suggested Stations are also available for observation of Egress.)



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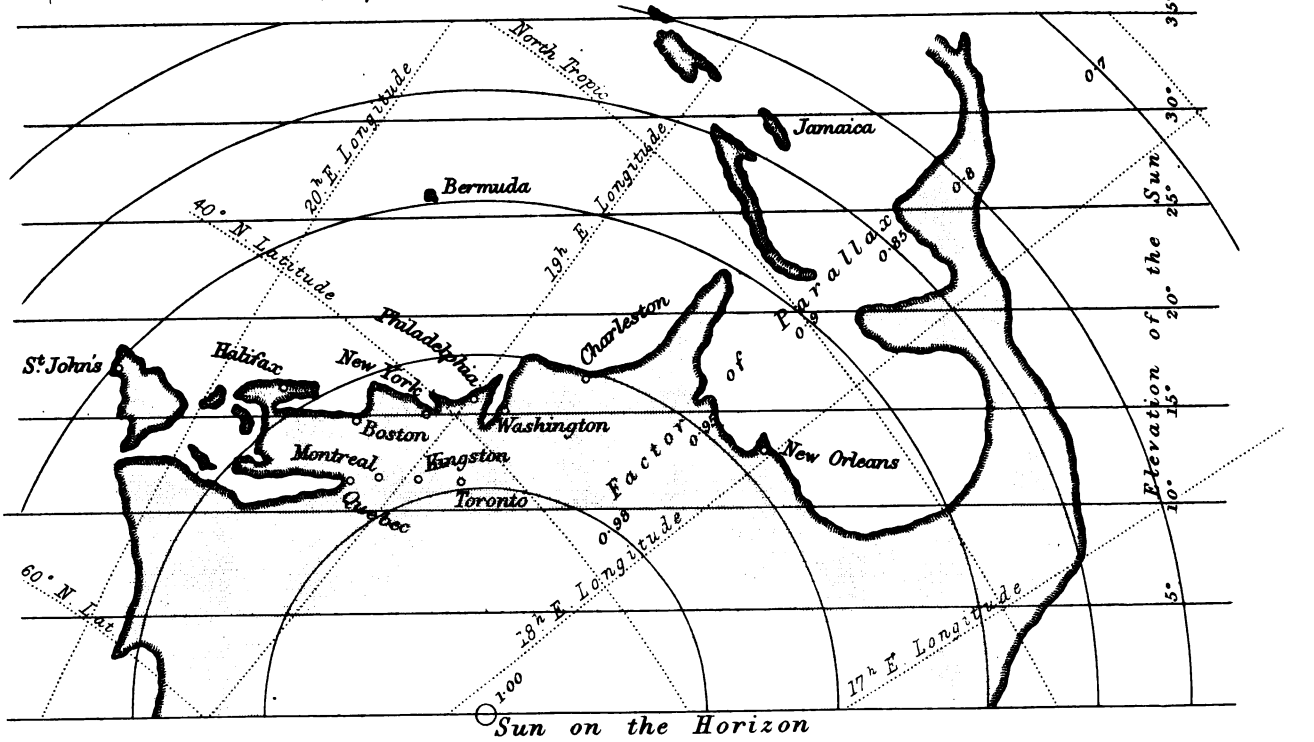
Transit of Venus, 1882, Dec. 6.

Stations for observation of the Ingress of Venus.

The Sun ascending. Venus entering on the Lower Limb.

Ingress retarded by Parallax.

(Note. The American Stations (except Newfoundland), and the Islands, are also available for observation of Egress.)



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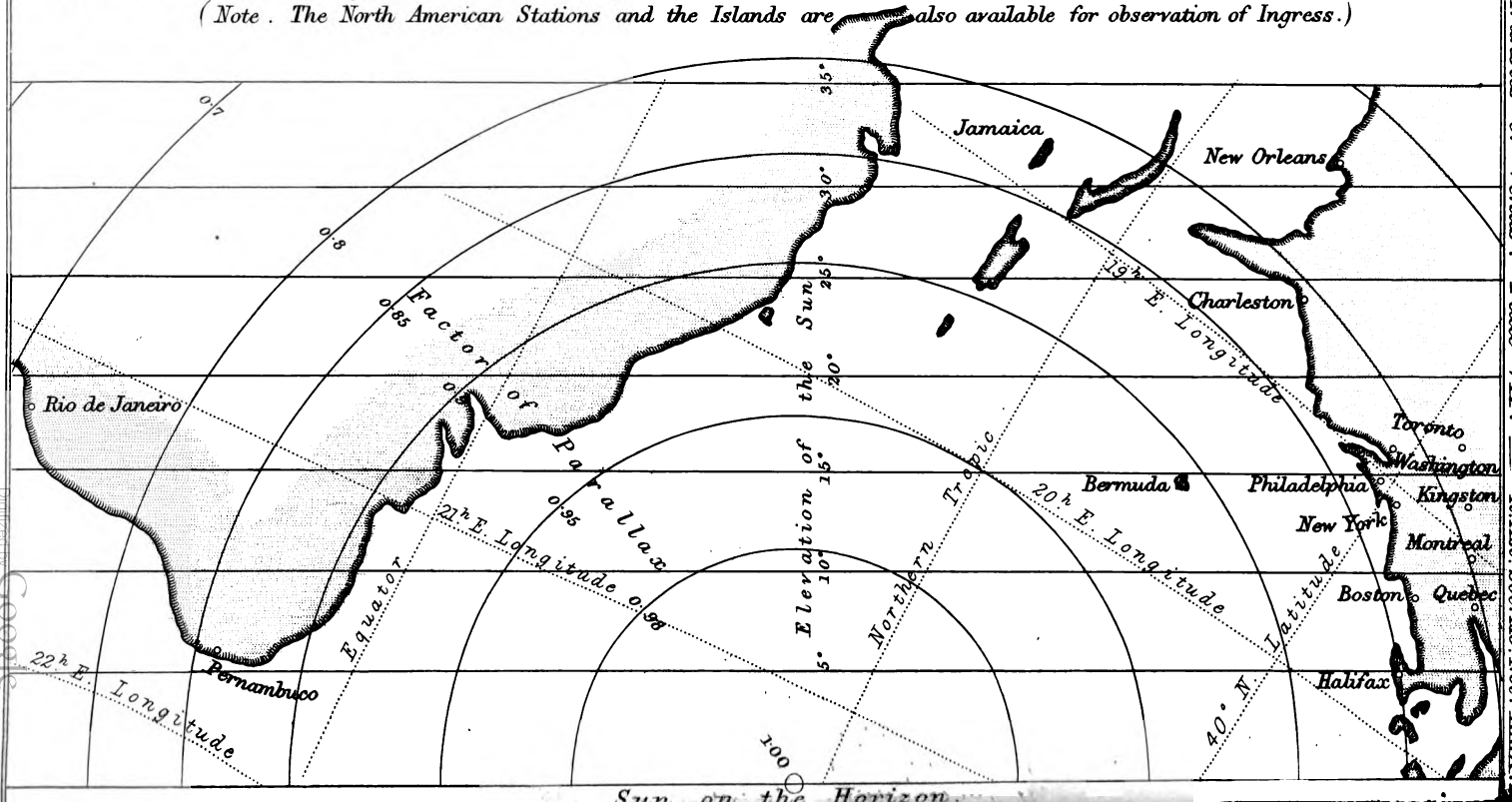
Transit of Venus, 1882, Dec.r 6.

Stations for observation of the Egress of Venus.

The Sun descending. Venus leaving the Lower Limb. Egress accelerated by Parallax.

(Note. The North American Stations and the Islands are also available for observation of Ingress.)



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Transit of Venus, 1882, Dec. 6.

Stations for observation of the Egress of Venus.

The Sun ascending. Venus leaving the Upper Limb. Egress retarded by Parallax.

(Note. The "Suggested Stations" are also available for observation of Ingress.)

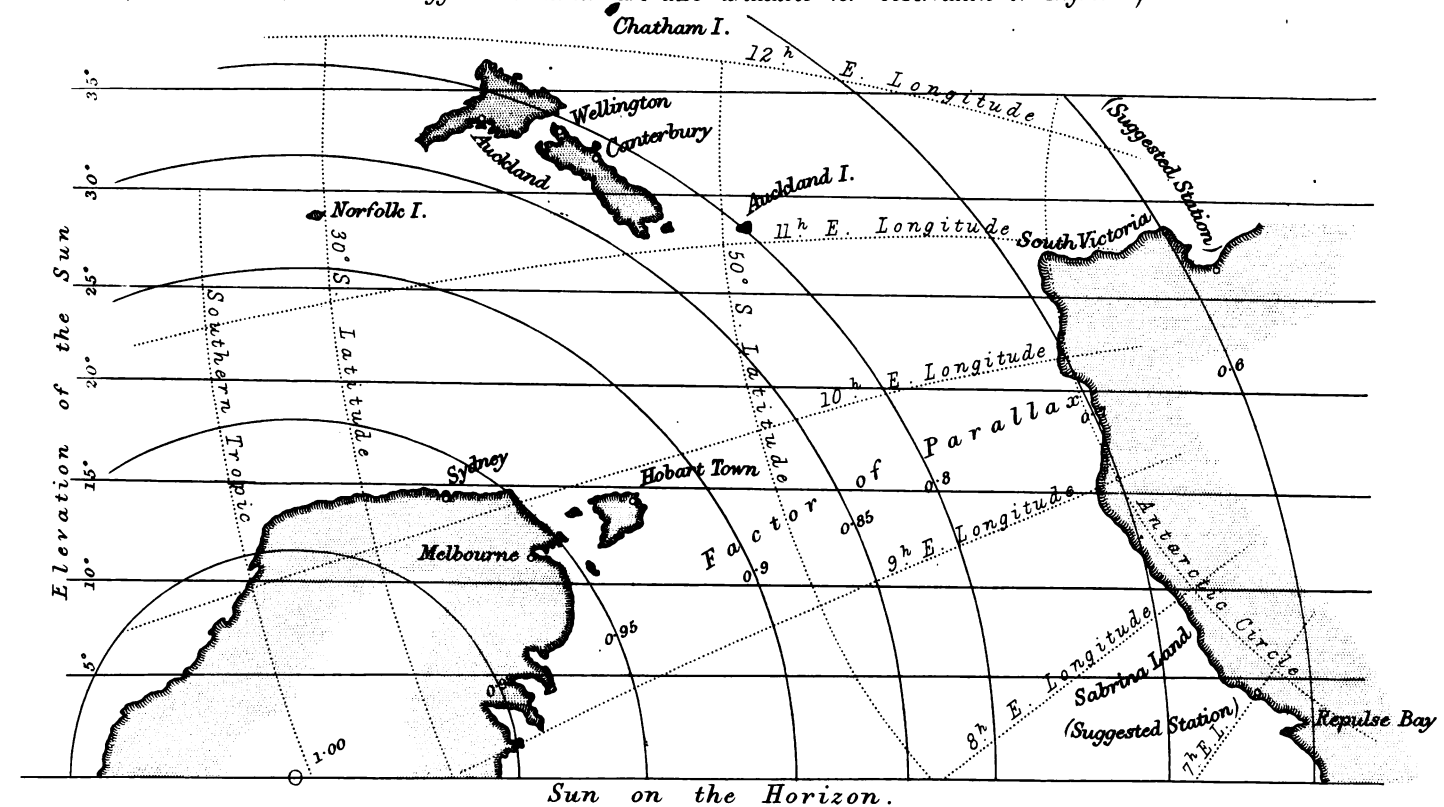

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Figure 1.
1874.

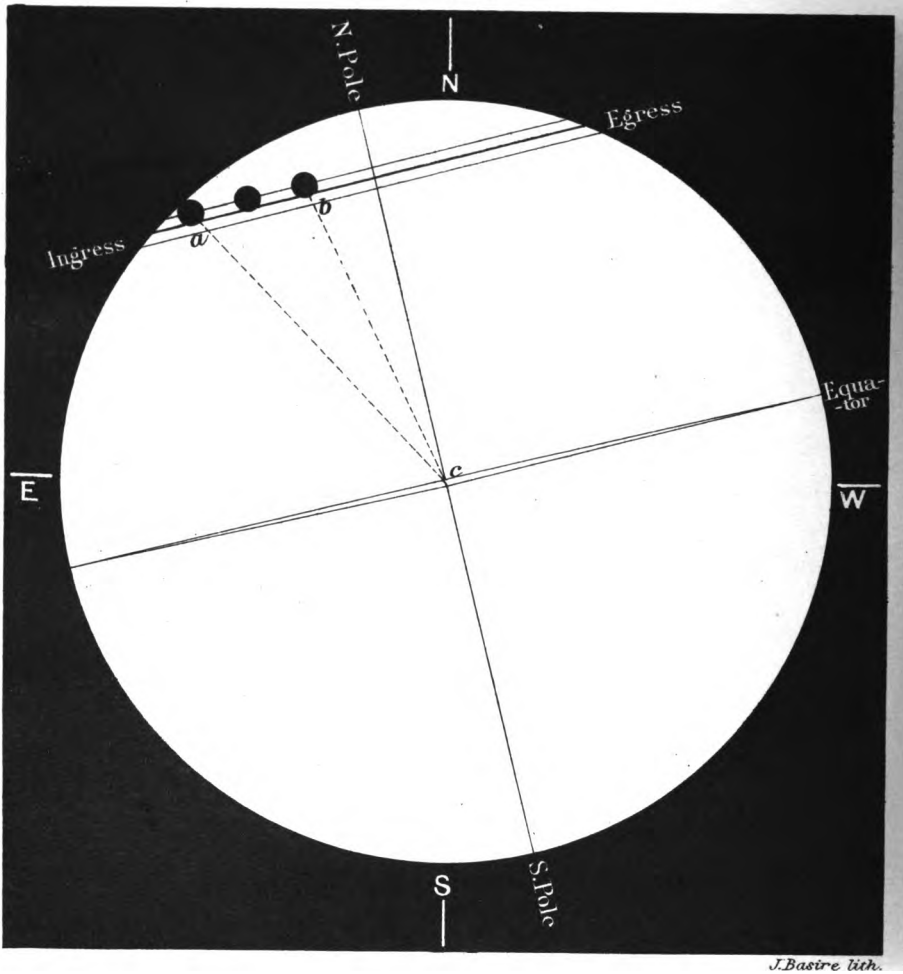

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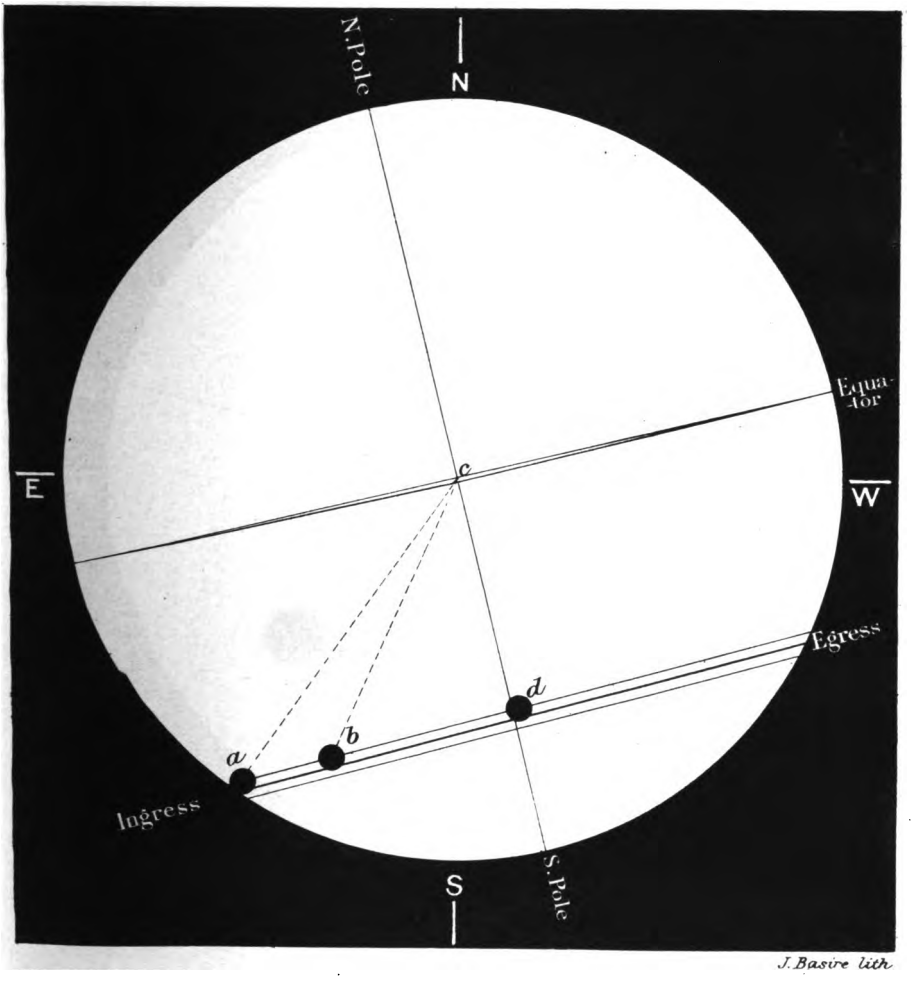
Figure 2.
1882.



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July or August, 1868, 7″·94. His drawings are accompanied by a scale of 8″, from which it appears that on the 26th of August the diameter of the crater was 1″·9. With these data we obtain the following dimensions of the features observed by Tacchini, viz. the crater and a small dark spot in the middle of the white spot. This dark spot he regarded as a central pit—

Diameter of Crater	= ·35737
West shoulder	= ·09404
Floor?	= ·15047
East shoulder	= ·11285
Central pit (orifice)	= ·03762

Assuming that the surface around the cone and probably within the crater is strewed with ejected material, more or less fragmentary, resembling *broken glass*, as Mr. Huggins has suggested, we have to a certain extent an explanation of the appearance of the white spot *upon* the disappearance of the shadow. At sunrise the greater inclination of the west slope will cause it to appear as a bright, well-defined spot; the east slope being very gradual, the sun soon attains an altitude greater than the inclination of the slope, and the bright spot becomes merged in one of more than double its diameter, immediately upon the disappearance of the shadows, which is very rapid. The observation of the disappearance of the shadow after sunrise and its reappearance before sunset will assume some degree of importance, as any change in these phenomena with regard to solar altitudes will indicate corresponding changes in the surface around the orifice. On the other hand, if the surface remain in the same quiescent state the shadows will disappear and reappear with the same solar altitudes.

Mädler to W. R. Birt.

"*Bonn, October 7th*, 1868.

"Dear Sir,—The joined copy of a letter from the parts of Mr. Tacchini, received 22nd September, with the original drawing accompanying it, will be, as I believe, a precious document regarding the crater *Linné*. Therefore I have the honour to send you both of them.

"I take this occasion to remark, that the great whitish spot surrounding *Linné*, which I have found in the English observations and in the drawing of Mr. Tacchini, is never seen by me in 1831. The crater was surrounded by the greenish colour of the *Mare Serenitatis*.

"We have finished the translation of the preface of my work of the Moon, which expresses clearly my opinion about its contents. I shall send it in the next to Mr. Goodwin, to look it over on account of the language, adding the request to send it to you directly. If only the meaning is clearly understood we think to go on with the translation."

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M. Tacchini to Professor Mädler.

"*Reale Osservatorio, Palermo, 16 Sept.* 1868.

"Dernièrement j'ai fait des observations sur le cratère *Linné*, et je me permets de vous en envoyer quelque détail, et même des dessins, que vous pourrez confronter avec les votres, puisque ce n'est qu'à une autorité aussi respectable que la votre qu'il peut être donné de juger sur les prétendus changements du cratère.

"Dans le nuit du 26 Juillet j'ai observé l'ombre intérieur du cratère, comme l'indique la fig. 1. Dans le 28 les conditions atmosphériques étaient encore bonnes, et dans le cratère je n'ai plus observé l'ombre aussi marquée, mais dans la partie occidentale on observait seulement une trace d'une légère pénombre, qui donnait encore l'indice d'une faible cavité, fig. 2; en profil le *Linné* serait donc comme la fig. 3, tandis que les autres cratères environnants avaient encore une ombre bien nette et noire. Dans le 29 le cratère apparaissait complètement blanc, alors j'ai porté à conclure que le cratère n'est plus dans les conditions enrégistrées par vous.

"Dans la lunaison suivante j'ai répeté mes observations, et j'ai été encore plus heureux. Dans la nuit du 24 Août le *Linné* était à peine sorti de l'ombre, voyez fig. 4. Dans le 25 le cratère était bien distincte, et le contour bien marqué à l'occident, et presque invisible dans la partie opposée. Dans le 26, avec beaucoup de surprise, j'ai vu encore la cavité de *Linné*, mais plus restreinte, et semblait une petite tache a demi-noire dans le milieu d'une tache blanche, fig. 5, mais jamais une ombre nette comme dans les autres cratères voisins, le terminator passait par *Timocharis*. Ce petit cercle sombre ne correspond donc pas à l'espace renfermé par le contour du cratère, mais il est un creux central; alors le profil de *Linné* serait comme la fig. 6. La tache blanche toute entière, c'est-à-dire avec le peute extérieure, serait selon mes mesures $7''{\cdot}94$.

"Dans votre lettre au R. P. Secchi vous dites que le cratère montrait l'ombre de l'intérieur encore, quand les autres cratères environnants en étaient dejà depourvues. Donc le cratère a changé véritablement de forme, du moins dans l'intérieur.

"Eh bien, d'après cela, je dois vous adresser une prière, peut être sera trop hardie, mais j'espère d'être favorisé pour le bien de la science.

"Pourriez-vous m'envoyer une copie de vos dessins? Je les publierais en lithographie, et alors la question sera, je crois, definitivement resolue, car avec des simples mots il est toujours difficile de s'entendre.

"Agréez, Monsieur, mes salutations bien distinguées,

"TACCHINI PIERRE,

"*Astronome adjoint de l'Observatoire de Palermo.*"

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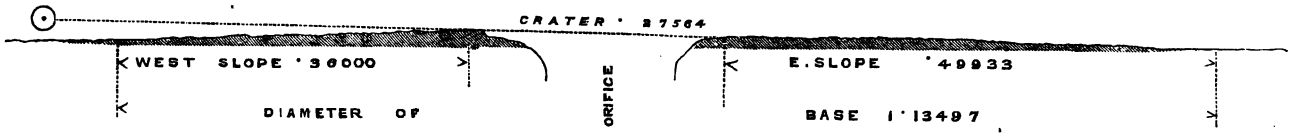
SECTION of *Linné*, from a Drawing by Mr. HUGGINS, 1868, June 26.

Length of Shadow = 1·00000.

Sun's Altitude at disappearance of external Shadow.



SECTION of *Linné* from Drawings by Mr. HUGGINS and Prof. TACCHINI.

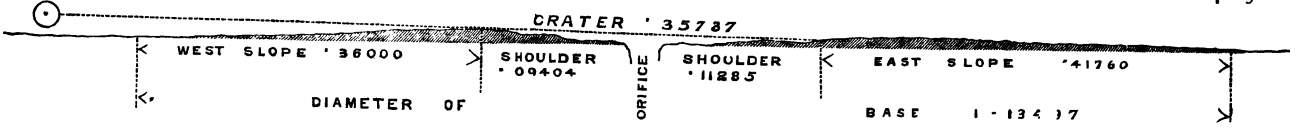
1868, June 26 and Aug. 26.

Measure of White Spots 7″·94.

Height of Cone, $\frac{14}{1000}$ of Shadow.

Sun's Altitude at disappearance of external Shadow 1° ±.

Length of Shadow = 1·00000. Sun's Alt. 0° 46′·5.



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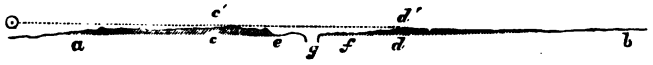

Section deduced from drawings by Messrs. Huggins, Noble and Tacchini, and a measure by Mr. Buckingham on June 26 Length of shadow = 5″·75. Sun's altitude, 1° ±.

	English miles.	English feet.
The Cone—		
Height of west rim *c c'*	..	492
Height of east rim *d d'*	..	265
Base *a b*	7·8	41282
West slope *a c*	2·4	13094
Crater *c d*	2·5	12998
East slope *d b*	2·9	15159
The Crater—		
West terrace *c' e*		3420·4
Floor *e f*		5472·9
East terrace *f d'*		4104·5
The Orifice—		
The central pit of Tacchini *g*		1368·2

Line of Section east and west nearly.

The dotted line from ⊙ represents the line of the Sun's rays at the time when the shadow on the exterior towards the east disappears.

Although the preceding deductions are presented not only in the form of, but are actually the results of, observation, and not taken from an *imaginary* section, yet they must be received as approximations only, serving rather as a guide to observers than as being expressions of the *exact* state of *Linné* at the present epoch. It is nevertheless probable that within certain limits the features above specified will be found normally correct.

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Elements of New Minor Planets. By Professor Watson.

I have determined the orbits of the new minor planets lately discovered by me, and the results are the following,—

Elements of Hecate (100).

Epoch = 1868, Sept. 1·0, Washington M.T.

$$\left.\begin{aligned} M &= 10^\circ\ 5'\ 30''.4 \\ \varpi &= 304\ 45\ 0.5 \\ \Omega &= 128\ 28\ 37.7 \\ i &= 6\ 33\ 34.6 \end{aligned}\right\} \text{Mean Equin. } 1868{\cdot}0.$$

$$\varphi = 8\ 39\ 32.5$$

$$\log a = 0.493331$$

$$\log \mu = 2.810010$$

$$\mu = 645''.669.$$

Elements of Helena (101).

Epoch = 1868, Sept. 13·5, Washington M.T.

$$\left.\begin{aligned} M &= 17^\circ\ 48'\ 53''.0 \\ \varpi &= 328\ 40\ 51.0 \\ \Omega &= 343\ 35\ 0.1 \\ i &= 10\ 4\ 19.5 \end{aligned}\right\} \text{Mean Equin. } 1868{\cdot}0.$$

$$\log a = 0.410460$$

$$\log \mu = 2.934317$$

$$\mu = 859''.640.$$

Elements of Planet (108).

Epoch = 1868, Oct. 12·5, Washington M.T.

$$\left.\begin{aligned} M &= 33^\circ\ 43'\ 52''.6 \\ \varpi &= 326\ 15\ 4.8 \\ \Omega &= 135\ 56\ 56.4 \\ i &= 5\ 21\ 35.2 \end{aligned}\right\} \text{Mean Equin. } 1868{\cdot}0.$$

$$\varphi = 4\ 37\ 37.6$$

$$\log a = 0.431728$$

$$\log \mu = 2.902415$$

$$\mu = 798''.758$$

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Elements of Planet (104).

Epoch = 1868, Sept. 13·5, Washington M.T.

$$M = 316^\circ\ 9'\ 43''.2$$
$$\left.\begin{aligned} \pi &= 62\ 11\ 55.4 \\ \Omega &= 43\ 46\ 42.1 \\ i &= 2\ 53\ 26.7 \end{aligned}\right\} \text{Mean Equin. 1868·0.}$$
$$\varphi = 11\ 22\ 53.3$$
$$\log a = 0.502401$$
$$\log \mu = 2.796405$$
$$\mu = 625''.756.$$

Elements of Planet (105).

Epoch = 1868, Oct. 12·5, Washington M.T.

$$M = 106^\circ\ 13'\ 49''.8$$
$$\left.\begin{aligned} \pi &= 242\ 36\ 17.8 \\ \Omega &= 187\ 54\ 1.8 \\ i &= 21\ 38\ 59.0 \end{aligned}\right\} \text{Mean Equin. 1868·0}$$
$$\varphi = 10\ 8\ 54.0$$
$$\log a = 0.376567$$
$$\log \mu = 2.985156$$
$$\mu = 966''.398.$$

Elements of Planet (106).

Epoch = 1868, Oct. 10·5, Washington M.T.

$$M = 346^\circ\ 44'\ 32''.5$$
$$\left.\begin{aligned} \pi &= 35\ 37\ 53.6 \\ \Omega &= 62\ 42\ 38.9 \\ i &= 4\ 41\ 33.2 \end{aligned}\right\} \text{Mean Equin. 1868·0.}$$
$$\varphi = 11\ 14\ 46.0$$
$$\log a = 0.505287$$
$$\log \mu = 2.792076$$
$$\mu = 619''.550.$$

Ann Arbor, 1868, *Nov.* 23.

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Observations of Jupiter's Satellites. By J. Joynson, Esq.

			h m s
1868, Sept. 1	I.	Sat. I. first contact	10 39 55 G.M.T.
		last do.	10 45 45
8	II.	Sat. Oc. R. noted	9 57 31
		last contact	10 1 25
	I.	Shad. I. noted	11 39 25
		full on	11 43 39
	I.	Sat. Tr. I. first contact	12 23 8
		last do.	12 29 24
	I.	Sat. Tr. E. last contact	14 36 17
Oct. 10	I.	Sat. Tr. I. first contact	8 9 26
		last do.	8 15 47
	I.	Sat. Tr. E. first contact	10 18 13
		last do.	10 23 28
	I.	Shad. E. first contact	10 32 41
		last do.	10 36 6
11	I.	Sat. Ec. Reappearance	7 47 59·9
17	II.	Sat. Oc. D. first contact	7 44 34
		last do.	7 52 24
	III.	Shad. E. first contact	7 58 30
		last do.	8 8 10
	I.	Sat. Tr. I. first contact	9 52 13
		last do.	9 57 18
	I.	Shad. I. noted	10 17 23
		full on	10 20 45
	II.	Sat. Ec. Reappearance	11 4 11·1
	I.	Sat. Tr. E. partly off	12 7 18
		Interrupted by cloud.	

Waterloo, near Liverpool.

ERRATA.

Monthly Notice for November 13, 1868.

Page 8, line 19 from bottom, *for* same contact, *read* some contact.

— 12, — 5 from bottom, *for* $22^h\ 2^m\ 35^s{\cdot}7$, *read* $21^h\ 2^m\ 35^s{\cdot}7$.

— 15, Observations by H. J. Carpenter, *for* 12^h, *read* 21^h.

— 17, line 16 from top, in reference to the phase of the egress observed by Mr. Dunkin, *for* figs. (2) and (3), *read* figs. (1) and (2).

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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VOL. XXIX. *January* 8, 1868. No. 3.

Admiral MANNERS, President, in the Chair.

Rev. R. H. Blair, Worcester;
E. Clodd, Esq., Edmonton;
Capt. Haig, R.E., Poonah;
Howell Jeffreys, Esq., Balliol College, Oxford;
James Knight, Esq., Glasgow;
Lt.-Gen. Lane, Sandown, Isle of Wight;
Rev. J. Saywell, Bromsgrove;
A. F. Smith, Esq., Training College, Borough Road;
E. J. White, Esq., Observatory, Melbourne; and
Lieut. Wither, Tangier Park, Basingstoke,

were balloted for and duly elected Fellows of the Society.

On the Solar Eclipse of August 18th, 1868.

By Warren De La Rue, Esq.

Under date of November 9th, 1868, Major Tennant sent to me, from Calcutta, enlarged paper copies, a little over two inches in diameter, of each of six photographs he had obtained of the eclipse; also a negative photographic enlargement of the Great Horn (which I call A), about nine times the size of the original negative; and, lastly, a photograph of a drawing of this great prominence made by Mr. James on a scale about three times the size of fig. 7.

With reference to these photographs Major Tennant makes the following remarks:—

"In each of the photographs the corner next the Great Horn (A) is very nearly where the *apparent* vertex is in an inverting telescope. The inclination of the wire (in Nos. 1 and 2) was 39° 15′ to the parallel, and the position angle of the horn is 75°."

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I suggested to Major Tennant, before he left England, that it would be advantageous, in order to refer the position of the prominences to the Sun's centre, to take enlarged pictures on glass, and to etch them according to the plan I showed him, and which I had used for the eclipse of 1860, and obtained, by the electrotype process, the copper multiples from which were printed Plates xiii., xiv., xv., &c., in the *Philosophical Transactions*, 1862. In reference to this suggestion, Major Tennant says:—"The large pictures were thin and poor, and there was no use in treating them like yours by etching; for the real thing would be better shown by a distribution of the transparencies, so as to make them generally accessible."

Major Tennant has accordingly sent eight sets of transparent copies on glass of his eclipse-pictures to the Astronomer Royal for distribution; one set of these is for me, and when they come into my hands* I intend to try and procure from them etched plates like those which I made of the eclipse of 1860, as I have more leisure than Major Tennant could command, and also appliances for doing so which he does not possess. In the meantime, however, I have worked on the paper copies, and although the results I have obtained must be considered merely as first approximations, I venture to think that they may have some interest for Astronomers, inasmuch as they afford data for referring the prominences seen at different stations to a meridian drawn through the Sun's centre, thus offering facilities for a complete study of the whole phenomena observed at the various stations along the line of the central eclipse.

The only prominence visible *throughout* the totality at Major Tennant's station at Guntoor was the Great Horn (position-angle 75° in reference to the Moon's centre). Prominence G first became visible at the epoch of No. 5 photograph; at the epoch of No. 4 only the Great Horn, A, was depicted. Prominence C is completely covered in No. 3, and prominence B partially so, as well as the soft light indicated by dotted lines in the diagram, close to B towards the north, position angle 116° to 125°, height above the Moon's limb 1′ 41″. The outline of this soft light is not changed in the interval between 2 and 3, and it cannot, on account of its well-defined form, have arisen from light reflected from the photosphere by the Sun's atmosphere. Another patch of light, with a softened but characteristic branching outline, is visible between the prominences B and C, position angle 133° to

* I received these photographs from the Royal Observatory on Jan. 22d, while this paper was passing through the press. They contain much more detail than is visible in the paper copies; for example, in No. 1, a band of low prominences can be seen in the midst of the glare, hereinafter alluded to, extending from position angle 96° right up to prominence B. In No. 2 is a very curious group of faint prominences K making up half an ellipse, from position angle 65° to 72°, the minor axis of the ellipse extending about 40″ above the Moon's limb. These pictures are extremely interesting, and, in my opinion, must be considered as eminently successful results.—Jan. 23, 1869. W. D. L. R.

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139°, height above the Moon's limb 2′ 27″. Similar appearances are indicated in my eclipse-pictures of 1860, and form part of the appendages of the Sun. These entities, bounded by outlines which blend almost imperceptibly into the general light of the corona, are deserving of especial study, which will be best accomplished by means of photography, as it truly records their faint contours, which are likely to be overlooked in eye-observations, because they are lost in the softened light which surrounds the Moon; and also because the eye is naturally attracted most by the prominences which have a distinct outline.

The wire of which Major Tennant speaks in Photographs Nos. 1 and 2 can be well discerned in the first, but is seen with difficulty in the second picture. In none of the paper copies can the Moon's limb be traced beyond a little more than the half of its circumference;* and, at first, it did not appear possible to discuss these pictures graphically in the same way as I had done my own photographs of 1860, but ultimately I was enabled to make out the diagram which I now lay before the Society; the work was accomplished in the following manner:—

By very careful measurements the centre of each picture was first found, and a circle representing the lunar disc then described with the dry point of the compasses.

Taking the angle of position of the Great Horn (which did not change greatly during the totality†) as 75° in all the pictures, the line N S was ascertained and also drawn with a dry point. A careful tracing was then made of the pictures 1, 2, and 6, and the tracings were in such a manner superposed that the Great Horn coincided in all, and that the lines N S, N′ S′, N″ S″, were seen to be as nearly as possible parallel. By these means the relative positions of the Moon's centre at the epochs of No. 1 and 6 were obtained. By drawing the circle to represent the Sun in such a position that the lunar peripheries at epochs Nos. 1 and 6 were tangential to it, the radius of the Sun was ascertained and the position of his centre at the same time laid down. It was then possible to obtain the following elements, as may be seen by an inspection of the diagram, which is a reduction, to about the same size as my original photographs of 1860, of a larger diagram, drawn out on a scale of half an inch to a minute of arc:—

Orbital motion of the Moon's centre during the totality, referred to the Sun, was a parallel to a diameter drawn from	285° to 105°
Orbital angle therefore	15°

* In the glass copies the Moon's limb can be traced nearly all round in photograph 5, and completely so in 6.

† This angle changed about 2° 50′ in reference to the two positions of the Moon's centre in photographs Nos. 1 and 6; but this was only ascertained as the diagram was developed; for this reason the lines N S and N″ S″ could not be made quite parallel when tracings of the photographs were superposed.

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Measured orbital motion during the interval between Nos. 1 and 6, namely $5^m\ 31^s{\cdot}9$, reduced to seconds of arc*	132″
The relative motion of the Moon's centre in Right Ascension during the interval between Nos. 1 and 6	W. to E. 128″
The relative motion of the Moon's centre in Declination during the interval between Nos 1 and 6	S. 32″·4
[The relative motion *during the totality*, namely in in $5^m\ 45^s$, would consequently have been:— in R.A. 133″·5 and in Decl. 33″·7.]	
Position-angle of 1st internal contact, or point where the light disappeared	101°
Position-angle of 2nd internal contact, or point where the light reappeared	294°
Nearest approach of centres	7″
Ratio of the Moon's semidiameter to the Sun's semidiameter = 1	1·07188

Prominence.	Position Angle referred to Sun's Centre.		Height above Sun's Limb.	Height above Moon's Limb.
	°	°	′ ″	′ ″
A	76·0 to	81·5	3 10	3 4†
B	123·0	127·0	1 0	0 54
C	139·0	148·5	1 32	1 19
D	262·0	263·0	0 22	0 13
E	283·9	287·5	0 54	0 54
F	295·5	298·5	0 44	0 44
G	314·5	322·0	1 7	1 0
H	326·0	331·0	0 45	0 32
I	335·5	337·5	0 48	0 27
K	65·0	72·0	...	0 40‡

In the diagram I have inserted the position of the Sun's spots and faculæ, as photographed at the Kew Observatory. §

Sun-spots do not appear to have had in August, 1868, more than in July, 1860, any immediate connexion with the prominences. A patch of faculæ, however, between position-angles 138° and 147°, extending up to the limb of the Sun, corresponds well with the position of prominence C. And the patch of faculæ 255° to 257° nearly so with the small prominence D.

Indeed, so far from anticipating that there could be any prominences surmounting Sun-spots or in immediate contiguity with them, the published researches in Solar Physics by myself, Mr.

* The value of the arbitrary scale employed was reduced to seconds of arc by taking the Sun's semidiameter to be 15′ 50″·6, and deducing therefrom the value of 1″ on the scale.

† These heights are exclusive of the curved point *a*, fig. 7, which cannot be seen in the paper photographs; its length is about 12″, making the total height of the Great Horn 3′ 22″ above the Sun's limb.

‡ Not shown in the diagram, as it is not visible in the paper copies.

§ *Monthly Notices*, vol. xxix. p. 3.

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Balfour Stewart, and Mr. Loewy, induce us to think that there ought to be an absence of these appendages where Sun-spots exist. The prominences are evidently more closely connected with the faculæ, for we now know with certainty that they arise from an uprush of heated gases, whereas we have stated that Sun-spots are closely connected with a downrush of comparatively cold gaseous matter.

The positions of the Sun's axis, equator, and two parallels of latitude, 30° N. and 30° S. in the diagram, are derived from the following data:—

	° ′
Inclination of the Sun's Axis to the East of the Meridian through the Sun's centre	17 24·9
Elevation of Earth above Sun's Equator	+6 52·6

Major Tennant, in a previous letter, communicated to me his observations of the spiral structure of the Great Horn, A, which was printed in the *Athenæum*, October 3, 1868, No. 2136, with some remarks of my own, in which I gave the credit to Major Tennant for having first pointed out this peculiarity of structure; but, at the same time, I called attention to the fact that a similar spiral structure was visible in my photographs of the Eclipse of 1860, for example in the prominences C and D, as depicted in my plates Nos. ix. to xv. *Phil. Trans.* 1862. In his letter of November, 1868, Major Tennant adds the following statement:—"About the Horn: I trust there can be no doubt about its structure, but I would draw attention to the closeness of the spiral low down, when first formed, and its size above, which seem to me to accord with my theory. Indeed, I almost feel that there are two close spirals in the lower part of the luminous vapour, each partaking of the general motion, and in the upper part coalescing. There should be, according to Dr. Vogel's account, a sensible change in the 5½ minutes, and I think I have mentioned that I suspected one before I saw his paper. It is quite clear that, from some cause, the latter photographs are taller than the first; what puzzles one is the white hole (Fig. 7 *b*). In No. 1 one sees the knob outside it; and in No. 6 a little of it is seen in the same place; as it is in all between these two pictures. At the time this seemed to me conclusive against a change, but I am disposed now to believe that this could be explained otherwise, and that the change of height is real, and not an accident of exposure entirely. My theory would require the change, but that was not formed until recently, long after my suspicion had been given up as too uncertain for evidence, and I did not hear of Dr. Vogel's result until after my paper was ready for dispatch."

The paper copies are not sufficiently distinct to enable me to ascertain whether there was any change in the aspect of the Great Horn during Major Tennant's observations,* but it is very

* From a cursory examination of the glass copies, I believe that they contain evidence of an axial rotation of Prominence A.—Jan. 23, 1869. W. D. L. R.

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probable that there was, for there is reason to suspect that this prominence was undergoing rotation; and I call attention in support of this view to the very different appearances of prominence A depicted by various observers along the line of totality.

I am dependent in the main on the representations of the phenomena given in the *Engineer*, of November 6 and November 13, for the appearances presented by the Great Horn (A) at Aden, on board the ships Rangoon, lat. N. 15° 42′, long. 59° 15′ E., at Beejapoor, at Wha Tonne in the Malayan Peninsula, and at Labuan. My friend, Mr. W. Fane De Salis, one of the Directors of the Peninsular and Oriental Company, kindly sent me, in September, 1868, a tracing of Captain D. Rennoldson's drawings, and I find that two of them are faithfully represented in figs. 14 and 15 of the *Engineer*, and this fact inspires confidence in the other pictures published in that journal. Fig. 2 is a copy of one of the tracings sent to me.

On reference to the subjoined diagrams, all drawn out to the same scale as that representing Major Tennant's photographs, it will be seen that in the Aden photograph, obtained on Aug. 17, $15^h\ 30^m\ 39^s$, the Great Horn was so curved that its point was directed northward, see fig. 1; in fig. 2, representing the appearance observed on board the steam-ship Rangoon, the direction is even more decidedly northward; this observation occurred at $15^h 39^m\ 18^s$, or nine minutes after that at Aden. In fig. 3, as observed at Beejapoor at $16^h\ 1^m\ 32^s$, after a further interval of 22 minutes, the point is directed southward. At Guntoor, the extremity of the Great Horn still pointed southward, but not so markedly; the appearance of it is represented in fig. 4, and occurred at $16^h\ 10^m\ 52^s$. So that it is probable that the Great Horn performed half a rotation on its axis between the observations at Aden and those at Guntoor, that is, in 40 minutes. Fig. 5 shows the appearance of the Great Horn as observed on the Malayan Peninsula, at about 17^h; in this case the extremity points northward, as at Aden, and it appears to have performed half a rotation in the interval between the Guntoor and Wha Tonne records, that is, in 49 minutes; and at Labuan, at $17^h\ 52^m$, the direction of the extremity is southward, and the prominence had possibly performed another half a rotation in 52 minutes, the velocity having progressively decreased. Between Aden and Labuan one-and-a-half rotation was apparently accomplished in a direction from left to right, looking from the Sun's centre towards the prominence.*

I am not aware whether the representation of the Aden observation in the *Engineer* was copied from an actual photograph or from a print which had appeared in another publication. The hypothesis which I indicate of the probable rotation of the Great Horn depends, however, very much upon the faithfulness of the woodcut in the *Engineer*, and until prints of the Aden photo-

* The direction of the rotation is inferred from that of the convolutions of the spiral of the Great Horn.

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graphs are compared with those of Guntoor it will not be possible to speak with certainty on the point, nevertheless I have thought it desirable to draw attention to the subject.

In Major Tennant's photographs, and in my own of 1860, the deep indentation of the concave side of the prominences is very striking, leading to the impression that they do not actually touch the photosphere, but are elevated at some little distance above it.* The interval must be extremely small, for in 1860 I did not notice any luminous ring between the Sun's limb and the prominences, nor was any depicted in the photographs; but at the recent eclipse there appears to have been remarked by more than one observer that there was an indication of such a brilliant ring of light. In Major Tennant's first photograph, between position-angle 80° and 120°, is a large halo (covering an area equal to about $\frac{1}{7}$ of the lunar disk), which prevented part of the surroundings of the Sun's limb from being clearly depicted, produced evidently by a strong light, and in reference to this he makes the following remarks in his letter of November 9, 1868:—

"I wish to call your attention to the bright edge of light in No. 1, which I believe to be the stratum of heavy luminous vapour. It may be said that there is a glare, and this may be the residual sun; but the cusps extend beyond the glare, and it would have been hidden only after 10 seconds, while Phillips says (what is confirmed by other testimony) that the Sun was lost very shortly indeed after the exposure. I am quite satisfied this was not the solar disk, and if there be any question about it, I hope you will be able to confirm my opinion."†

Under date of November 28, 1868, Major Tennant, in relating a conversation he had had with M. Janssen, says, "His account of the passage of the Moon's disk over the edge of the Sun puzzles me. As he puts it, the bright surface in my No. 1 must have been the Sun itself, but if so, the limb must have been very pale beyond all I ever saw. He, however, said he could not be certain that a narrow line did not exist, when I pressed him."

I cannot from the examination of the paper copies say with certainty whether the bright glare was or was not due to the residual Sun, but I rather inclined at first to the belief that it was not due to the Sun.† This opinion was founded on my experience of 1860, when a very minute crescent of the Sun was taken, in No. 28 photograph, when the versed sine of the Sun's cusp was only 9″·6, by an extremely rapid uncovering and recovering of the object-glass; the instantaneous slide in the Kew heliograph not having been brought again into action for the partial phases after it had been removed for the totality pictures, until the epoch of the next succeeding photograph. Although, in the case of the Kew heliograph, the image, in consequence of its enlargement by

* In the glass Photograph 6 are depicted several perfectly detached prominences which are not seen in the paper copies.

† A careful examination of glass photograph No. 1 has convinced me that the glare was not due to a cusp of the Sun.—Jan. 23, 1869. W. D. L. R.

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the secondary lens, was reduced in intensity 64 times, yet the plate was so solarised that no trace of prominence A, which was visible at the time, was depicted; whereas in No. 1 of Major Tennant's photographs very minute bead-like prominences near the Great Horn, together with this and the prominences B and C, are clearly recorded.*

The position angle (namely 111°·5) of the centre of the glare is 10°·5 to the South of the first internal contact, and this halo could not have been caused by the last traces of a cusp of the Sun, which would have been to the *North* of the first internal contact, but a furrow on the Moon's limb may have allowed the passage of a minute beam of the Sun's light, and this would have been quite sufficient to have produced the glare, and I think this is a probable explanation of the appearance, but I reserve for a future communication my final opinion on this point.

In the account given by Capt. Haig, R.E., in the *Proceedings of the Royal Society*, No. 105, 1868, of his observations near Beejapoor, he gives the following extract from the notes of Capt. Tanner, who took part in them:—"As regards the corona, when we first began to see the Eclipse through the clouds, I was under the impression that the eclipse, instead of being total, was only annular, so bright was the corona near the Moon's limb. I could not detect any irregularities in the light of the corona, but the light appeared to be gradually shaded off all round." In this bright light are evidently two distinct phenomena mixed up together, namely, the brilliant self-luminous entities with softened outlines, before spoken of, and the true corona caused by the reflection of the Sun's light by his non-luminous atmosphere.

I quote from the *Engineer*, November 13, 1868, page 371, with respect to the observations of M. Stéphan on the Malayan Peninsula: "After the side of the Sun had disappeared the Moon was seen by M. Stéphan and M. Tisserand as bordered by a thick luminous ring for about a quarter of a minute; this ring was brighter than the surrounding sky, and was sufficiently brilliant to cause an error as to the exact moment of the contact. It reappeared a few seconds after (before?) the third contact."

Although the extent of the ring of light is evidently estimated at far too large a quantity for it to be situated beneath the underside of the prominences or to have escaped depiction in the photographs, and is, in all probability, due to the same causes as the phenomenon described by Capt. Tanner; yet it would appear that there are still some doubts left to be settled, and that, notwithstanding the means we now possess of studying the Sun's appendages when he is not eclipsed, we must wait for future total eclipses before we can ascertain with certainty whether there

* On the occasion of the reading of this paper, Serjeant Phillips was present, and he stated that in his opinion the glare might have been caused by residuary sunlight (for the signal to expose the plate was given to him before the Sun completely disappeared), and that he purposely delayed doing so for a few seconds, until he judged by the *unassisted eye* that the Sun had become covered.

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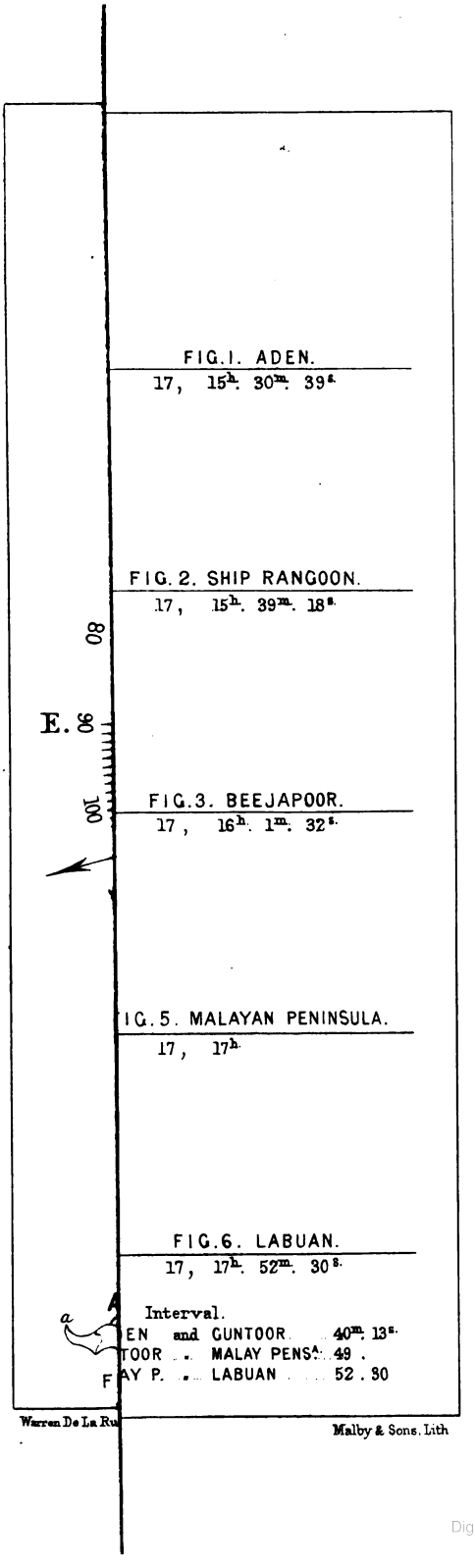

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is any self-luminous atmosphere between the photosphere and the underside of the prominences.

It will, on future occasions, be extremely desirable to have photographic observations made as nearly as possible on both the Northern and Southern limits of the shadow as well as at stations situated near its centre.

Moreover, it would be very desirable to have two precisely similar instruments at the same station, in one of which the sensitive plates would be exposed for 1 second, and in the other instrument from 30 seconds to 1 minute, in order to obtain data for the correct interpretation of all the phenomena.

In connexion with the observations of the Sun's appendages when it is not eclipsed, it may be permitted to recall to recollection my estimates of the relative brightness of the prominences and the solar disk,* namely, that the former were in 1860 from 696 to 1111 times less brilliant than the Sun; I think this will be confirmed when the relative dispersion of the Sun's light and that of the prominences by the spectroscope is calculated; this of course depends on the dispersive power of the prisms and the opening of the slit. If, for example, the solar spectrum is dispersed over a space of 10° or 600′, and the bright lines occupy together about 2′, the light of the prominences would only be three times less brilliant (according to the mean of the estimates I have quoted above) than the Sun, and might in consequence be seen on the spectrum of the disk, which we know is possible; it would of course be much more brilliant than that of the spectrum of our atmosphere in contiguity with the Sun.

Mr. Hind has, since this paper was written, been so good as to give me the following data, calculated for the assumed position of Major Tennant's station :—

Guntoor.	
Longitude	80° 27′ East.†
Latitude	16 15 North.
Middle of Eclipse, 1868, Aug. 17,	16h 10m 52s M.T. at Greenwich.
Duration of totality ..	0 5 45
Sun's semidiameter ..	15′ 50″·6
Moon's apparent or augmented semidiameter	16 59·0
Position-angle of first internal contact, or point where light first disappeared	103° N. towards E. direct image.
Position-angle of last internal contact, or point where light reappeared	71° N. towards W. direct image.
Nearest approach of centres	3″·9
Moon's apparent hourly motion in R.A.	23′ 27″·3 E.
,, ,, Decl.	6′ 32″·1 S.

* From two different sets of considerations, *Phil. Trans.* 1862, p. 405.

† The position given in Major Tennant's paper is —

Long. E. 80° 27′ 9″; Lat. N. 16° 17′ 29″.

[Jan. 29, 1869. W. D. L. R.]

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Moon's apparent Decl.	13° 3′ N.
Orbital angle is therefore	15° 58′

A comparison of the data deduced from Mr. Hind's numbers and my own is subjoined:—

	Hind.	De La Rue.
Position angle of first internal contact, N. E. S.W.	103°	101°
Position angle of second internal contact ..	289°	294°
Nearest approach of centres	3″·9	7″
Moon's apparent motion during the totality in Right Ascension E.	134″·86	133″·5
Moon's apparent motion during the totality in Declination S	37″·59	33″·7
Ratio of semidiameter of Moon to semidiameter of Sun = 1	1·07195	1·07188
Orbital angle	15° 58′	15°

The data obtained graphically will be found to be not very discordant from those calculated, especially when it is taken into consideration that they were obtained from paper photographs on so small a scale as those at my command.

The Society has received from Major Tennant a detailed Report* of his Observations of the Solar Eclipse of 18 August, 1868, as seen at Guntoor, with illustrations, including the photographs and drawings referred to in the foregoing paper by Mr De La Rue: also the copy of a Report addressed to Sir Alex. Grant, Bart., Director of Public Instruction, Poona, by Mr. Kero Laxuman Chatrey, Prof. of Mathematics and Natural Philosophy at the Deccan College, of the Eclipse, as observed by him in company with Captain C. T. Haig, R.E., and Captain Tanner, at Beejapoor; the account by Captain Haig of these observations has been published in the *Proceedings of the Royal Society*. The Society has also received a communication from Col. T. Addison as to the observations of the Eclipse at Aden.

The Great Nebula round η Argûs. By Lieut. J. Herschel and Sir J. W. F. Herschel, Bart.

(*Observations of the Nebula by Lieut. Herschel, and Remarks by Sir J. Herschel, in reference to the supposed Variation of the Nebula.*)

Lieut. Herschel, in a letter to Sir J. Herschel, dated Bangalore, November 23, 1868, writes:—

* Jan. 29, 1869. I have this day had an opportunity of reading this most valuable and interesting paper, and am glad to say that my interpretation of the appearances depicted in the photographs agrees in many particulars with that of Major Tennant.—W. D. L. R.

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I have this morning had a look at the Nebula round η *Argûs*. Had I known I could see it so early I should have tried sooner. It is now on the meridian (and therefore has an altitude of 20°) at sunrise, and I can see it readily two or three hours earlier.

You will understand that I do not mean to content myself with one morning's examination, and that if I send you a hasty account, it is, in some measure, to record *first impressions* purposely.

The Nebula is *easily* seen with the naked eye. The eye *selects* the object as naturally as it would choose the Pleiades at that altitude. I have not compared it *directly* with Nubecula Major (preceding by six hours), but as I happened to have looked at the latter two nights before, I may trust my memory to say that the Nebula η *Argûs* is undoubtedly intrinsically brighter.

In a binocular it appears as two elliptic patches.

The figure is from memory.

I examined it telescopically with powers of 55, 85, 125, 200, sketching the appearance in each case. These sketches I have now before me. You know how difficult it is to represent faithfully one's impression of a nebula by a hurried pencil drawing, and will understand that the accompanying *copy** (based on all) has no pretension to accuracy. *In fact it is a wretched attempt.*

There is a copy (?) of your drawing in *Chambers's Astronomy*. This I had by me; and compared with my first and second sketches after they were made, not to correct my impression or sketch, but to fix the former and to note the agreements and disagreements in the latter. I did not look at your figure (in the *Outlines of Astronomy*) until I came in.

On the whole, sketch for sketch, I see no more marked differences than I should see in a similar comparison of a sketch of the Nebula *Orionis* with your figure (on the same page).

I marked the positions of twenty-nine stars in the central part only, of 9 to 12 or 14 mags. Of these I recognized the positions of seventeen at least, as practically identical with those in your figure, four are smaller than the run of those you show, three are so close to η that the brilliancy of that star no doubt obscured them. The remaining five are unidentified and unrepresented. η itself I estimate (ignorantly) at 7 to 8 mag.

I *do* recognize differences of configuration, no doubt, chiefly in the bridging of the dark space, and partial filling up of one portion; also the presence of another space scarcely more than indicated in your figure.

Upwards of fifty Nebulæ of different degrees of brightness

* Afterwards referred to by Sir J. Herschel as the sketch marked A: see Plate A.—Ed.

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have been under inspection, during the last few nights, and I think I may say that they have to some extent qualified my eye to judge of shape and relative brightness; and lastly to state that, so far as an hour and a half's study of this particular one is concerned, I do not recognize such a total "subversion of all the greatest and most striking features &c.," as Mr. Abbott's diagrams and statements appear to imply.

No doubt the relative brightness of some of the stars has altered; but, so far as I have compared, not remarkably (except η of course).

I propose to make a more precise survey of both stars and Nebula at once.

Sir J. Herschel, in a letter to Admiral Manners, dated Collingwood, Dec. 22, 1868, communicating the foregoing, writes:—

The extreme interest created by the account communicated by Mr. Abbott of the changes which would seem to have taken place in the great Nebula about η *Argûs* leads me to conclude that it would be wrong to delay communicating to the Royal Astronomical Society a letter which I have just received from my son, Lieut. J. Herschel, whom I requested to examine it so soon as its time of meridian passage would allow, with a view to ascertain, so far as the instrumental means at his command would admit, the nature and extent of the alleged changes, and at the same time to note the configuration of the stars surrounding the principal one (η). The letter is accompanied by two sketches—eye-drafts—the one (that marked A) being the one referred to more especially in his letter; the other (marked B) is not therein expressly referred to, nor indeed have I any positive ground for certainty that it is a sketch of that Nebula at all, further than that he speaks of having made sketches with each of the powers used, of one of which, probably that with a low power, I suppose it to be a copy; though it presents so little resemblance to the other as to allow of a very reasonable supposition (in the absence of any such guiding circumstance) that it had been taken for some other nebula. Indeed I should not have inclosed it at all, but as an instance of the very different impression one and the same nebula may make when viewed with different optical powers.

As the telescope with which the sketch (A) was made inverts differently from the 20-feet reflector with which my drawings were made, I annex a semi-reversed copy of (A) [marked (C)],* formed by pricking through the stars of (A), and then reversing the paper, and copying in with as much precision as possible the nebulous surroundings (semi-reversed) on the back of the paper. I have also added allineations pointing to the stars unmistakably identified, twenty in number, annexing the letters and numbers

* The semi-reversed copy (C) is not here reproduced; but see the plate (A A), which exhibits a like copy of Lieut. Herschel's second sketch of the Nebula.—Ed.

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which stand attached to them in my Catalogue of the Stars of the Nebula. Of these I ought to mention that two, viz., (α) No. 736 and (γ) No. 741, whose magnitudes are rightly stated at 9 and 11 in the Catalogue, are represented much too small in the engraving, though in their right places.

Unidentified are the four stars which form a pentagon with H towards η. There are eight small stars in that area in my chart, and these are doubtless four of them not laid down with sufficient precision for the purpose of identification.

The unidentified star marked ? ? δ is a very important one. If it be really that star (a star 10 mag.) the evidence of change in the Nebula would seem to be really conclusive, since that star, when I delineated the Nebula, was carefully laid down as a landmark for future reference, as being just within (by a distinct though minute interval) the sharply-defined southern border of the Lemniscate, while three other exceedingly minute stars, with a similar view to future reference, on the south preceding border, were *just without* or *just on* the edge. But the allineations of the hypothetical δ disagree very markedly with those of the real δ, which lies in an exact straight line between C and (ζ), as also between D and G. The former condition is pretty well satisfied, but the latter is very wide of the mark.

On the whole, allowing for difference of optical power, there is no difficulty in recognizing the Lemniscate form, and my son's drawing, at all events, is decidedly adverse to the utter breaking up of its well-defined southern border by two clear open channels, which forms so marked a feature in Mr. Abbott's delineation.

Further and more elaborate delineations will undoubtedly follow.

P.S. On further consideration I am disposed to consider the sketch (B) as an attempt to delineate the clustering stars intermixed with nebula in the south preceding branch of the Great Nebula.

Lieut. Herschel's further observations, dated Bangalore, 29 Nov. 1868, are as follows:—

I examined this Nebula on the mornings of the 22nd and 23rd November with the Royal Society's 5-inch refractor, using different powers from 55 to 200, and sketching the appearance presented in each case. The second morning was devoted to one careful survey of the stellar points in and about the central portion, and to one sketch, based on it, of the nebulous light. The accompanying sketches show the appearances under powers of 55 and 125, both being drawn *afterwards* from the sketches made on the spot (the originals being retained, as I may be enabled hereafter to secure further observations). I believe they are both very faithful, but the appearances are so different, not only under different magnifying powers, but also under a more or less sustained gaze, that it is difficult to avoid giving features too marked a character,

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on the one hand, and to indicate them correctly on the other. The relative intensity of the broader features is no doubt best seen under a low power, but the fact that in that case marked outlines are not traceable, which come out stronger under a high power, seems to indicate a tendency to exaggerate the latter effect. I am inclined to attribute somewhat of the differences *in this respect* between the present sketch and the figure of this Nebula in the "*Outlines*" to an endeavour on my part to avoid the difficulty by giving an intentional indistinctness to some parts about which I could not feel any confidence. It is hardly necessary to say that I carefully avoided comparing the above until I had completed my own.

Out of about sixty stars plotted, I believe not more than five or six are erroneously placed (to the extent of affecting appretiably the configuration). Of the rest by far the greater portion are easily identified with stars of similar magnitudes in the older drawing; a few are strangers, and perhaps two or three of these are (now) of sufficient magnitude to attract attention as not represented in the published diagram. In one or two cases also where there are *pairs*, their direction is more or less different; but this is hardly more than might be expected in thirty years.

The nebulous area, of which the accompanying low-power sketch forms the principal portion, is visible to the naked eye as a nucleus in the milky way. The latter appeared to me, on the morning of the 24th November, as of more intrinsic brightness than *Nubecula Major*, height for height; and of course therefore there was no comparison between the latter and the Nebula. It is an object which the eye selects at once for closer examination. Under a low power (of 3 or 4) it looks like two oval patches with many stellar points. Of these only one is shown in my sketch (2), being the portion which was naturally examined first.

There does not appear to be any very remarkable change in the distribution either of the stars or the Nebula, so far as that part immediately round η is concerned; but, if that portion formerly represented all that could be recognized in a moderate telescope, a great change must be acknowledged, arising, as it would seem, from the increased relative intensity of surrounding parts, rather than from subversion in the central.

It seems only necessary to add that the altitude at the times of observation was about 15°, and that the air was clear enough to have shown stars of the third magnitude within a degree of the horizon, roughly speaking.

Sir J. Herschel's letter, dated Collingwood, 2 Jan. 1869, communicating the foregoing, is as follows:—

Herewith I have the honour to enclose two drawings which I have received from my son, Lieut. J. Herschel, being those, I presume, alluded to in his first brief communication as intended

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to be executed with greater care; as also a description accompanying them, which, if not too late for communication to the Royal Astronomical Society at their next meeting, might very advantageously be considered simultaneously with the former drawings and their accompanying comments.

I accompany my son's drawings with two others, which will serve the purpose of elucidation. The first, marked (A A), is formed by pricking off the stars laid down in his figure No. 2, of November 24, through the existing pinholes in this figure; and shows the direction of the meridian and orientation in that figure as it must have been at the time of making the sketch. It exhibits also the letters and numbers in my Catalogue of the stars in and near the Nebula with which those in his drawing are identified. Of all the stars so pricked off, forty-nine in number,* there is only one of whose identity there can be any doubt, viz., a minute star indicated in both our figures by a query ? attached. It is either the star 14th magnitude, marked as *r*", No. 792, in my Catalogue considerably misplaced, or the star No. 650, of 17th magnitude, with which its place agrees. If his (?) is intended to express a doubt as to the actual existence of a *suspected* star in that place, the latter supposition must be adopted.

The other accompanying chart, marked (B B), is formed by pricking off all the stars in my large engraving within the compass of the paper, and then, at the back of the paper so pricked through, lettering and numbering in ink the identified stars, and those only. The semi-reversion thus effected brings the stars of this chart (when placed with the pencil line drawn through η *Argûs* and O parallel to a line through the same stars in Lieut. Herschel's figure) into correspondence as to right and left, top and bottom, with the rest of the stars in that figure, and their agreement (allowing for the difference of scales), will at once be evident.

On this chart I have also drawn in pencil the leading lines of the Nebula as it existed in 1834–38, from which I think it will be very evident (allowing for the great difference in the illuminating powers of the instruments employed) that the Lemniscate *still exists* and holds as nearly as might reasonably be expected under the circumstances the same relative position among the stars, and more especially as relative to η *Argûs* itself. In this drawing of my son's it should be noted that the *true* (δ) makes its appearance quite unmistakably, and it stands just where it ought, a little beyond the border of the Lemniscate *within the Nebula*. It is true that the outline of the Lemniscate is shown much less sharply defined, and, holding the drawing at a distance and in a good light, it may be clearly traced. The star (η), too, I may observe intermediate between (5) and (6), has by some inadvertence been

* In his description he speaks of "about 60" originally "plotted." I can find, however, only 50 in this figure, one of which accidentally lies beyond the border of the paper in my projection.

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omitted, but is unequivocally shown in its proper place in the drawing previously sent.

A decisive proof of the absence of any material shifting of position between the Lemniscate and *η Argûs* is afforded by the small star marked [*v*] in a direction pointing from *η* to about half-way between C and D. This star (No. 664 of my Catalogue) occupies, both in my engraving and in my son's drawing, precisely the same strongly-marked position close to the very edge of the most characteristic projection of the Nebula, being ***just immersed*** in the Nebula in both.

The drawing No 1, with a low magnifying power, it should be observed, is on a scale only about one-third that of my engraving, and of course is intended to take in not the Lemniscate region only but the great diffused outlying masses. These, as will create no surprise to those accustomed to the difference of appearance in Nebulæ under great differences of illuminating power, offer hardly any recognizable similarity to the curious and complex detail exhibited in my engraving. Yet the Lemniscate, as a small *closed* form to the left of the small star (*η Argûs*, the middle one between the three large stars laid down), is perfectly recognizable, though much more feebly marked than I should have expected. Of these three stars I presume the two upper ones in the figure to be W and Y of my chart. That in the lower portion is beyond its limits.

The plates belonging to the foregoing paper are—

1. Sketch A, in Lieut. Herschel's first communication.
2 and 3. The drawings 1 and 2, in Lieut. Herschel's second communication.
4 and 5. The tracings A A and B B, referred to in Sir J. Herschel's second letter.

Description of an Automatic Transit Instrument.

By Sidney B. Kincaid, Esq. (Abstract.)

It is stated that the object of the paper is to describe an apparatus for the adjusting of isolated timepieces, so simple in its construction as neither to be very expensive to provide at first, nor easily liable to derangement when fixed, and so simple in its mode of use, that it might be mastered in the course of an hour by any person of moderate intellectual powers.

The apparatus consists of a plane mirror and a burning-glass to be adjusted in such manner that, at the instant the Sun reaches the meridian of the locality, the rays ignite a thread which burns

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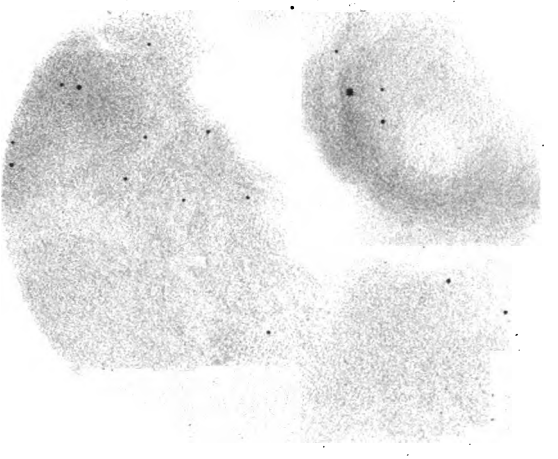
The Nebula round η Argûs, I.

(A)



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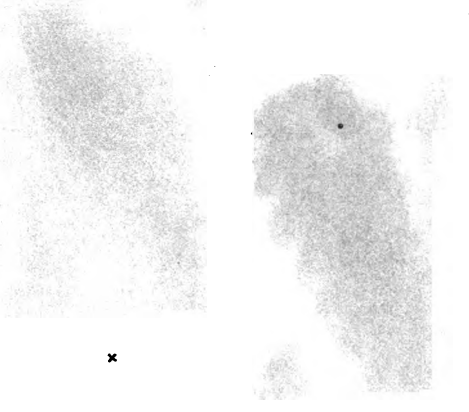

Approximate Length of One degree

1. GENERAL APPEARANCE OF THE NEBULA ARGÛS AS SEEN NOV^{R}, 23RD & 24TH, 1868, WITH APERTURE 5 INCHES, AND MAGNIFYING POWER 55.

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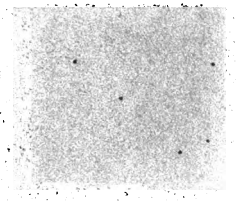

2. A PORTION OF THE NEBULA ARGÛS COPIED FROM SKETCHES AT THE TELESCOPE: APERTURE 5 INCHES: EYEPIECE 125.

J.H. 24/11/68.

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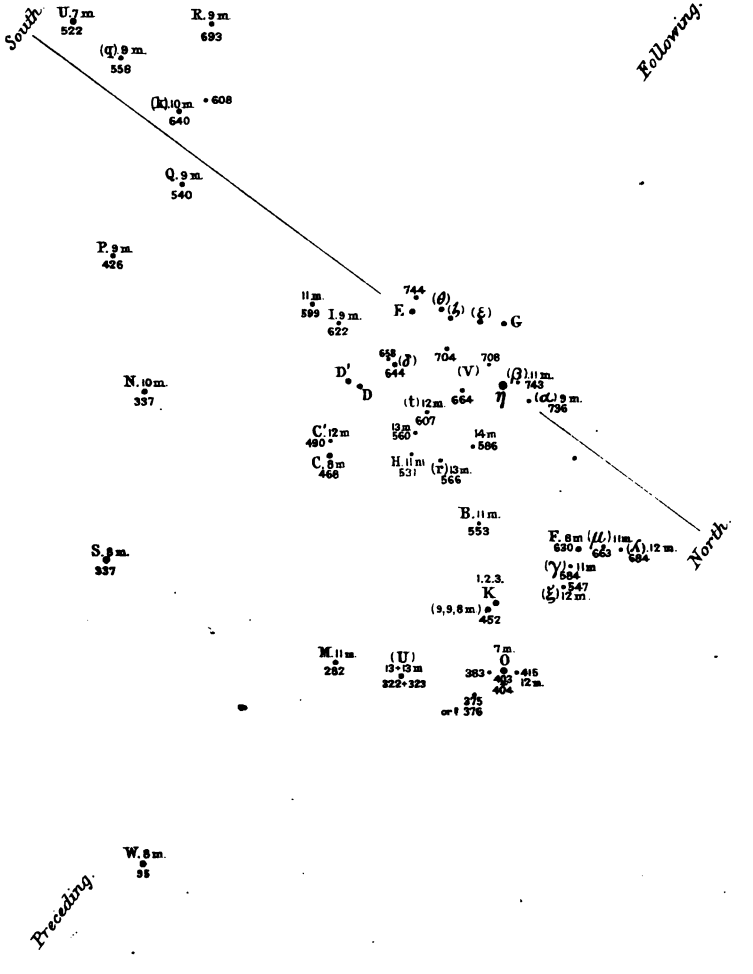
The Nebula round η Argûs, IV.

J.F.W.H. (A.A.).



IDENTIFICATION OF STARS PRICKED OFF FROM LIEUT. J. HERSCHEL'S DRAWING, MARKED 2 OF THE 24TH NOVR., 1868.

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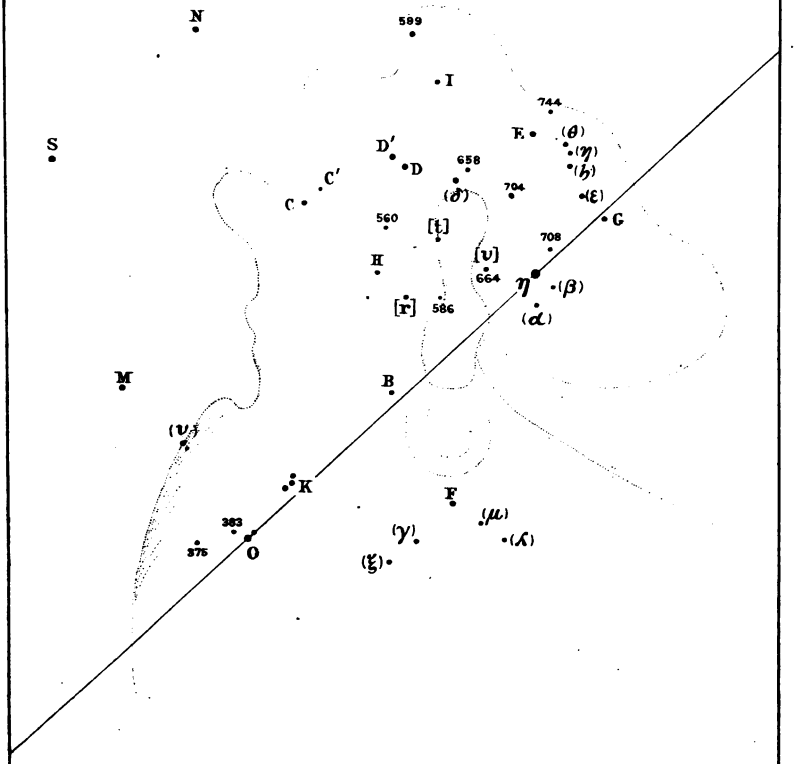
The Nebula round η Argûs, V.

J.F.W.H. (B.B.).



SEMIREVERSED BY PRICKING OFF FROM SIR J. HERSCHEL'S ENGRAVING OF THE NEBULA SHEWING THE TIME AND SITUATION OF THE IDENTIFIED STARS IN LIEUT. HERSCHEL'S SKETCH OF NOVR. 24TH, 1868, AND THE LEADING LINES OF THE NEBULA AMONG THEM, AS IT EXISTED IN 1834 — 1838.

The Oblique Line through η & O corresponds to the upright Sides of Lieut. Herschel's diagram.

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without smoke or residue; this releases a detent, and a motion is thereby given to the hands of the clock, bringing it to the correct local mean time. There is a supplementary arrangement by which, if, in consequence of a passing cloud, the thread does not ignite at the instant of the transit of the first limb, the subsequent ignition of the thread will not affect the clock.

The Transit of Mercury of Nov. 4–5, 1868. By C. Todd, Esq.

(Extract from a Letter to the Astronomer Royal.)

The weather being fine and beautifully clear I was able to take the following observations of the transit of *Mercury* on the 5th instant, which may perhaps be useful for purposes of comparison. I had previously calculated the angle from vertex of ingress, and looked for the planet at the right point; the time of first contact was, however, late a few seconds. The time of internal contact at both ingress and egress may be taken as correct, although the Sun was low down and rather ill defined at the egress. The observations were taken with a good Dollond's telescope of $2\frac{1}{2}$-inches aperture.

Ingress.

	h	m	s	
a. First contact	2	35	20	Adelaide Mean Time.
b. Internal contact	2	41	30	,,

Egress.

c. Internal contact	6	13	53	,,
d. Planet bisected	6	14	59	,,
e. External contact	6	16	3	,,

a. Seen late. Sun's limb slightly indented.

b. Good.

c. Considered good, but limb uneven.

d. Only approximate.

e. Sun's limb seemed perfect, but time may be few seconds too soon, the Sun being low and ill defined.

Ellery only got ingress.

I have made a good galvanic determination of the difference of longitude between Adelaide and Melbourne. I have fitted contact springs to my transit clock, and converted an ink-writing Morse instrument, by Siemens, into a very good Chronograph.

Electric Telegraph Department,
Office of Superintendent,
Adelaide, Nov. 11, 1868.

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Note on Mr. Huggins' paper "On a Possible Method of Viewing the Red Flames without an Eclipse." By J. Norman Lockyer, Esq.

In the paper referred to I read as follows:—

"The observations of the eclipse of August last having shown the position in the spectrum of the bright lines of the red flames, Mr. Lockyer and Mr. Janssen succeeded independently by a similar method in viewing the spectra of these objects."

The obvious meaning of the paragraph is that my work was based upon, and that my success was due to, the observations made during the eclipse in India.

I therefore beg permission to show (I.) that my work was not based upon the Indian observations, and (II.) that my success was in no way due to them.

I. In the first place I may remark that my work was begun in 1866, and the method I adopted was clearly indicated in a paper communicated to the Royal Society in that year. My observations were then unsuccessful owing to the small dispersive power of the instrument employed, which I have since ascertained was utterly incapable of showing the bright lines.

Secondly, I attempted to remedy this state of things by using an instrument of greater dispersive power, and I obtained a grant for the construction of such an instrument, which was ordered in January, 1867.

Thirdly, after a long series of unfortunate delays, due to instrument-makers and to my own severe illness, the instrument was received in an incomplete state on the 16th of October last (Friday night).

Fourthly, the discovery of the prominence spectrum was made on the 20th of October (Tuesday morning), *i.e.* four days after the instrument was received, and on the very first day I gave to the observations after the instrument had been mounted, adjusted, and tested.

Fifthly, my method of observation was to clamp my telescope on a particular part of the Sun's limb, and then to sweep along the spectrum very slowly (three adjustments being necessary) from red to violet, then to clamp on another part and work back, and then to begin again.

II. We now come to the second point. In what I have hitherto stated I have shown that the coincidence in time between my own results and the receipt of the information from India *was due to the date on which the new instrument was received.* I am convinced that had the new spectroscope been received a year ago the discovery would have been made just as easily; and Father Secchi's remarks on the ease of the observations, even with an instrument of moderate dispersive power, strengthen this view. I have also stated my method of work.

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Now let us see how the Indian observations were the cause of my success, or if indeed they could have caused the success of any one who depended upon them.

On the 20th of October I knew of three sets of observations: Rayet's, Herschel's, and Tennant's.

Rayet gave	B	D	E	*b*	F

and four more undetermined lines.

Herschel gave	near B or C	D	near F

declaring against F, and hesitating very much to assign even an approximate place to the line in the red.

Tennant gave	C	D	*b*	near F

believed he also saw a line near G.

The above table will, I think, show how utterly useless the Indian observations were for the purpose Mr. Huggins has assigned to them. In fact, every lettered line, except A and H, is named. Had I indeed worked with them at all I should naturally have looked for the line D, as that is the only line in which all the observations correspond. I distinctly referred to hydrogen (= C and F) in my paper of 1866, but in 1868 only one observer is sure of C, and only one observer is sure of F, and now it is stated that my success is due to the Indian observations.

I have sent this paper to the Royal Astronomical Society, not because any credit due to M. Janssen and myself is lessened by Mr. Huggins' paper, but because I know from experience that its *Monthly Notices* are one of the most important sources from which the history of Astronomy is written. I naturally therefore prefer that a true account of the recent discovery should appear in the *Monthly Notices*, instead of a statement incorrect in fact, which, were it left unnoticed, would mislead those who come after us.

On the Relation of the Luminous Prominences to the Faculæ of the Sun. By Prof. E. W. Brayley.

In the *Monthly Notices* for November 13, 1868, is inserted a paper on the Sun-spots and general aspect of the Sun on the day of the total eclipse of August 18, communicated by Messrs. De La Rue, Stewart, and Loewy. The large groups of spots visible, one near the eastern, the other near the western limb, were connected with faculous matter which apparently extended beyond the visible disk on both sides. The angles of position of these two parts having been given, the authors remark, "How far these

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positions agree with any observed protuberances can only be gathered after some time, when the whole of the detailed accounts have come to hand."

Mr. De La Rue does not refer, on this occasion, to a similar observation of his own which is recorded in his Bakerian Lecture on the Eclipse of 1860. With the view of ascertaining whether any connexion exists between the luminous prominences and the faculæ, or the spots on the solar disk, he obtained photographs of the Sun on the only five days, from the 14th to the 20th of July, on which the state of the weather permitted.

The faculæ surrounding a certain group of spots "extended evidently," Mr. De La Rue states, "beyond the visible portion of the Sun's surface on the 18th; for a part which was not in sight on the 18th came into view on the 19th and 20th. Just in the neighbourhood of these faculæ there was visible in the telescope during the totality *a very brilliant sheet of light*."* The significance of this last fact will appear hereafter.

The consideration of the facts recorded in the paper by the Kew observers reminded me of some observations cited in a *resumé* of recent researches on the Sun contained in three papers on its physical constitution which I contributed to the *Companion to the Almanac* (Knight's) for the years 1864, 1865, and 1866. Among these are some of great importance in relation to the present subject.

We have evidence that the terms *luminous prominences* and *faculæ* merely designate different portions, the inferior and the superior respectively, of the same physical objects. For this we are principally indebted to the photographic researches of Mr. De La Rue, the results of which I shall proceed to interpret.

In the first of the papers just alluded to, inserted in the *Companion*, for the year 1864, p. 45, I stated that the prominences might be regarded with some probability as arising from the exterior flames and condensed vapour or smoke "of the faculæ or their crests;" though I had not then recognised the force of the evidence on the subject which existed even at that time.

Mr. De La Rue had inferred from his own observations, both telescopic and photographic, of the prominences as seen in 1860, that they are far more generally distributed on the solar disk than the spots, being indeed very widely scattered over the Sun's surface. I afterwards stated (*Ib.*) "We must infer from the great extent of the limb they occupy, in profile or section, that they extend over the whole surface of the Sun." I did not mean as a continuous stratum (to which idea I have no claim), but merely as a collection of detached clouds, as they were then assumed to be, or detached ranges of clouds in the same plane as it were, forming a discontinuous spherical shell above the photosphere and the absorptive composite atmosphere incumbent upon it.

* *Phil. Trans.* 1862, pp. 395–396.

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By placing photographic pictures of portions of the Sun's surface in the stereoscope, Mr. De La Rue ascertained that the faculæ, as he inferred, occupy the highest positions of the photosphere; and "in one case," he relates, "parts of the faculæ were discovered to be sailing over a spot, apparently at some considerable height above it."*

Now the first of these results agrees with the fact that the luminous prominences are the most exterior phenomena of the Sun which are seen upon or above the limb; the second exactly describes the appearance which a floating prominence would have in a photograph viewed by the stereoscope; the actinic power of the prominences in general having been found by Mr. De La Rue to be very great. The prominences are the only phenomena of the Sun otherwise known which are seen detached from the edge of the disk. This result agrees also with the facts that some of the prominences are detached, and at a distance from the edge of the disk equivalent to the altitude of many thousand miles above the photosphere, which we know to be attained by many of the prominences which appear to be still connected with the edge.

I have stated, in the first article on the Sun already referred to, after Mr. De La Rue, that he entertains the hope of obtaining the outline of the prominences ("in plan, as it were, in solar horizontality," I have added for the sake of perspicuity), "'as very delicate dark markings,'" now to use his own words, "'on the more brilliant mottled background of the photosphere,' which, by the use of the stereoscope, will be discriminated from the other markings of the Sun's surface, and appear in their true position." It is remarkable that Mr. De La Rue did not recognise the fact that the photographic observation of the floating facula already cited was in reality an example of the fulfilment of this hope. What other appearance could one of the detached luminous prominences when thus observed present? It might indeed have been continuous with the spot over which it was seen, if the intervening matter were transparent, and that this was actually the case is manifest from the observation itself.

We have thus the strongest evidence that the faculæ and the luminous prominences are identical, or at least that the latter are the superior terminations of the former. If such be the fact, they must also be connected with the spots. I shall endeavour to evince, in another paper, that it will be consistent with all known facts to believe that they are the prolonged summits, rising from the level of the photosphere, of the torrents of matter originally wholly gaseous, ascending from the nucleal regions of the Sun, which, piercing through the successive envelopes of the nucleus from the cloudy stratum to the photosphere, cause the sun-spots; that the observed height to which the prominences ascend is at once the consequence and the index of the enormous force with which the original torrents are projected from the nucleus; and

* *Phil. Trans.* 1862, p. 407.

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that these inferences are not inconsistent with the local character of the spots.

London Institution, Jan. 8, 1869.

Addition, January 23, 1869.—Since the reading of this paper, it has been objected that, in consequence of the brilliancy of the photosphere exceeding by some hundreds of times that of the luminous prominences, while it is itself exceeded by that of the faculæ, the phenomenon observed by Mr. De La Rue—"the bright appendage of the Sun," which sailed "over the spot"—could not have been a prominence.

In the paper above announced, however, I shall adduce reasons, founded on Mr. De La Rue's own data, combined with the evidence of the spectroscope, for believing that in the stereoscopic projection of photographic pictures of the Sun's disk, the prominences, notwithstanding their inferior intrinsic brilliancy, may be expected to be seen as *bright* and not as *dark* markings, in agreement with my interpretation of the photographic appearance in question.

It has also been objected that the luminous prominences cannot be even partially identical with the faculæ, because the continuous spectra of the latter, forming part of the solar spectrum, proves them to consist, not of aëriform, but of solid or liquid materials (probably in the state of incandescent cloud), while the prominences have now been shown to be simply gaseous.

To this I shall only reply, at present, that, at the temperature of the photosphere, the faculous matter (especially if it consists of solid or liquid hydrogen), must be perpetually undergoing resolution into the aëriform condition at its upper surface, and, therefore, that while the spectrum of the lower parts of the faculæ-prominences will be continuous, that of their upper portions will be monochromatic, in accordance with the observed facts. But to this subject also I shall return in the sequel.

A communication has been received from Comm. D. E. Ashe, Director of the Quebec Observatory, stating that, since the date of his paper, "On the Physical Constitution of the Sun," *Monthly Notices*, vol. xxvi., p. 61, he had had his Equatoreal (aperture 8 inches, focal length 9 feet) fitted for Celestial Photography by merely inserting a sheet iron tube, in which is fixed the tube of a "Voigtländer" Camera, giving a picture of the Sun 3·4 inches in diameter, at a distance of 14 feet from the object-glass, and that the photographs are very good — two negatives sent to the Astronomer Royal showing all the phenomena of the surface, even the willow-leaves or granulations. The author's observations have confirmed him in the opinion, and he adduces further arguments

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in support of the theory that the Sun-spots are planetary bodies of small dimensions, originally revolving between *Mercury* and the Sun, and which have fallen upon the Sun.

Comparisons of Sun-spot Observations made in Dessau and Kew Observatory in the year 1868.

Communicated by Messrs. De La Rue, Stewart, and Loewy.

	Dessau.					Kew Observatory.				
Months.	Number of New Groups.	Numbers of the Groups.		Days of Obs.	Days without Sun-spots.	Number of New Groups.	Numbers of the Groups.		Days of Obs.	Days without Sun-spots.
January	2	No. 1 to	No. 2	19	10	2	No. 787 to	No. 788	6	4
February	2	3	4	21	2	5	789	793	9	2
March	8	3	12	27	0	8	794	801	12	0
April	8	13	20	25	0	6	802	807	12	0
May	7	21	27	31	1	5	808	812	21	0
June	9	28	36	28	2	15	813	827	19	2
July	8	37	44	31	7	8	828	835	21	4
August	8	45	52	31	0	7	836	842	16	0
Sept.	13	53	65	30	1	13	843	855	12	0
Oct.	13	66	78	28	0	17	856	872	16	0
Nov.	11	79	89	19	0	8	873	880	7	0
Dec.	12	90	101	11	0	21	881	901	13	0
Total	101	No. 1 to	101	301	23	115	No. 787 to	901	164	12

REMARKS.

The numbers of the groups in the Kew list are in continuation of the Catalogue given by Messrs. De La Rue, Stewart, and Loewy, in their "Researches on Solar Physics," part i.

The cause of the discrepancy in the total result is the discontinuation of the observations in Dessau since December 15, in consequence of the illness of Hofrath Schwabe. The latter part of December was especially rich in new and large groups, and there is every reason to believe that we are approaching rapidly the Sun-spot Maximum.

A very interesting phenomenon, viz. an indentation in the Sun's limb produced by a spot V, group No. 807, was for the first time simultaneously observed by the Kew and Dessau observers on May 7. The phenomenon was discussed by Messrs. De La Rue, Stewart, and Loewy, and the discussion has been ordered to be printed in the *Transactions of the Royal Society*.

Kew Observatory,
Jan. 7, 1869.

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On the Nebular Hypothesis, and the Approximate Commensurability of the Planetary Periods. By Daniel Kirkwood, LL.D. Professor of Mathematics, Indiana University.

The views of Laplace in regard to the development of our planetary system have been confirmed in a remarkable manner by the recent progress of astronomical discovery. We have now (1868) 100 minor planets between *Mars* and *Jupiter*, of which 96 have been detected since 1845. The mean distance of *Flora*, the innermost member of the group, is 2·20; that of *Sylvia*, the most remote, 3·49. The breadth of the zone is, therefore, greater than the distance of the Earth from the Sun; greater even than the entire interval between the orbits of *Mercury* and *Mars*. Moreover, the *perihelion* distance of *Sylvia* exceeds the *aphelion* distance of *Harmonia* by a quantity equal to the interval between the orbits of *Mars* and the Earth. The Olbersian hypothesis of the origin of these bodies seems thus to have lost all claim to probability. The only remaining theory worthy of consideration is that of Laplace, proposed in 1813, soon after the discovery of the first member of the group. "The double fact," says Plana, "of the multitude of those bodies and of their circulation in the same direction around the Sun, is now too imposing to admit an explanation of their origin and formation different from that developed by Laplace, in his *Système du Monde*." *

The known facts, however, in regard to the zone of the minor planets, as well as the phenomena of *Saturn's* rings, seem to demand a modification of the Nebular Hypothesis as generally held. No reason has ever been assigned why the solar nebula should not have abandoned rings at distances intermediate between the present orbits of the planets. On the contrary, it seems highly probable that, after first reaching the point at which gravity was counterbalanced by the centrifugal force arising from the rotation of the contracting spheroid, a continuous succession of narrow rings would be thrown off in close proximity to each other, and revolving in different periods according to Kepler's third law. It would seem, moreover, as shown by Mr. Trowbridge,† that the solar nebula, during the process of ring formation, was very oblate, and also much condensed at the centre; while, according to Prof. Ennis,‡ the rotation of the nucleus was extremely slow.

The theory of an almost continuous separation of matter at

* *Note sur la Formation probable de la Multitude des Asteroïdes qui, entre Mars et Jupiter, circulent autour du Soleil.* Par Jean Plana. Présentée le 2 Mars, 1856, à l'Académie Royale des Sciences de Turin.

In a subseqnent paper, by the same distinguished astronomer, it was shown that the mean inclination of the 62 minor planets known in 1861 was 8° 25′; exceeding the mean inclination of the solar equator to the ecliptic (7° 9′) by only 1° 16′. This coincidence has been rendered still more close by later discoveries; the mean inclination of 100 asteroids being almost exactly 8°.

† Silliman's *Journal*, Nov. 1864, pp. 350–353.

‡ *Origin of the Stars*, section xix.

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the equator of the solar spheroid is sustained by the fact that the zone of minor planets has evidently resulted, not from a single primitive ring, the parts of which moved with nearly the same angular velocity, but from *an indefinite number of narrow annuli.* The rings of *Saturn* may be regarded as another striking index to the process of planetary formation. With the exception of two permanent intervals, they appear to consist of small discrete masses, revolving in different periods according to their respective distances.

Why did not the Asteroid-zone form a single planet, and the rings of Saturn, one or more satellites? — In regard to the ring between *Mars* and *Jupiter*, perhaps no satisfactory answer can yet be given. We may remark, however, (1), that being situated just within the orbit of *Jupiter*, the perturbations were greater than in any other part of the system; and (2), that, as the total mass of the asteroids is very small, the matter of the primitive annulus was extremely rare, so that the intersection of orbits, resulting from the perturbations, would be less likely to produce large planetary nuclei. The rings of *Saturn* were doubtless comparatively dense. It has been shown, however, by Professor Vaughan,* that in positions so near the primary the force of gravity would render satellites of any considerable magnitude unstable. This force alone would, therefore, prevent the collection of large masses in the planetary form near the central body.

As these parts of the solar system, viz. the rings of *Saturn* and the minor planets between *Mars* and *Jupiter*, appear to furnish strong arguments in favour of the nebular hypothesis,† their phenomena may perhaps be further suggestive as to the *mode* of planetary formation.

The mean distance of a planet having —

A period equal to	$\frac{1}{2}$	that of Jupiter is	3·2776
,,	$\frac{4}{9}$	,,	3·0299
,,	$\frac{3}{7}$	,,	2·9574
,,	$\frac{2}{5}$	,,	2·8245
,,	$\frac{1}{3}$	,,	2·5012
,,	$\frac{2}{7}$	,,	2·2569

These distances all fall between the greatest and least mean distances of the asteroids. Now, do we find wider intervals in these portions of the ring than elsewhere? and if so, what is their physical cause? For the purpose of comparison we have, in the following table, arranged the minor planets in the order of their mean distances from the Sun.

* *Proc. Amer. Assoc. for Ad. of Sci.* 1856, p. 111.

† "No part of the solar system affords so striking an argument in favour of Laplace's Nebular Hypothesis as the Saturnian system of rings."— Proctor's *Saturn and its System*, p. 201. Plana's views in regard to the asteroids have been already quoted.

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Order of Disc.	Name.	Distance.	Order of Disc.	Name.	Distance.
8	Flora	2·2014	78	Diana	2·6228
43	Ariadne	2·2034	23	Thalia	2·6250
71	Feronia	2·2654	27	Fides	2·6422
40	Harmonia	2·2677	15	Eunomia	2·6429
18	Melpomene	2·2956	51	Virginia	2·6486
80	Sappho	2·2971	66	Maia	2·6539
12	Victoria	2·3342	26	Proserpine	2·6556
27	Euterpe	2·3468	85	Io	2·6594
4	Vesta	2·3613	72	Clytie	2·6648
30	Urania	2·3642	97	Clotho	2·6661
24	Clio	2·3674	3	Juno	2·6687
52	Nemausa	2·3779	75	Eurydice	2·6698
9	Metis	2·3862	77	Frigga	2·6719
7	Iris	2·3862	64	Angelina	2·6783
88	Echo	2·3939	24	Circe	2·6839
68	Ausonia	2·3972	58	Concordia	2·6979
25	Phocea	2·4023	55	Alexandra	2·7076
20	Massilia	2·4093	60	Elpis	2·7147
67	Asia	2·4209	45	Eugenia	2·7159
44	Nysa	2·4242	38	Leda	2·7399
6	Hebe	2·4254	86	Atalanta	2·7487
63	Beatrix	2·4287	28	Minerva	2·7554
21	Lutetia	2·4354	72	Niobe	2·7555
42	Isis	2·4401	82	Alcmene	2·7603
19	Fortuna	2·4414	1	Ceres	2·7660
79	Eurynome	2·4433	41	Daphne	2·7674
11	Parthenope	2·4526	83	Thisbe	2·7687
17	Thetis	2·4737	56	Pandora	2·7692
46	Hestia	2·5178	2	Pallas	2·7700
89	Julia	2·5498	89	Lætitia	2·7710
29	Amphitrite	2·5544	99	Leto	2·7748
91	Ægina	2·5584	74	Galatea	2·7777
0	Astræa	2·5772	28	Bellona	2·7784
13	Egeria	2·5775	81	Terpsichore	2·8563
32	Pomona	2·5831	33	Polyhymnia	2·8646
14	Irene	2·5895	48	Aglaia	2·8831
47	Melete	2·5976	22	Calliope	2·9091
54	Calypso	2·6101	16	Psyche	2·9263
70	Panopea	2·6132	50	Danae	2·9746

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Order of Disc.	Name.	Distance.	Order of Disc.	Name.	Distance.
(66)	Hesperia	2·9949	(10)	Hygeia	3·1494
(85)	Leucothea	3·0060	(21)	Euphrosyne	3·1561
(96)	Ægle	3·0549	(90)	Antiope	3·1576
(98)	Arethusa	3·0829	(87)	Mnemosyne	3·1597
(50)	Pales	3·0861	(94)	Aurora	3·1629
(58)	Europa	3·0999	(92)	Undina	3·1917
(49)	Doris	3·1044	(76)	Freia	3·3877
(68)	Semele	3·1119	(65)	Cybele	3·4205
(81)	Erato	3·1309	(97)	Sylvia	3·4927
(24)	Themis	3·1420			

Remarks on the foregoing Table.

The mean between the distances of *Flora* and *Sylvia* is 2·8470; and as small bodies in the remoter part of the ring are more difficult of detection, the zone will be considered under two divisions. The inner section contains 72 of the 97 asteroids whose elements are known; the mean interval between them being 0·0081. The greatest gap in the order of distances occurs between *Ariadne* and *Feronia*. It is 0·0620, or nearly eight times the mean. This includes the distance at which seven periods of an asteroid would be equal to two of *Jupiter*. The chasm next in order of breadth is between *Thetis* and *Hestia*. This is 0·0441, or more than five times the mean. In the outer section the mean interval is 0·0286. The greatest hiatus is between *Undina* and *Freia*; the breadth being 0·1960, or more than eight times the mean. This includes the distance at which two periods of a planet would be equal to one of *Jupiter*. The second is between *Bellona* and *Terpsichore* (0·0779), and the third between *Leucothea* and *Ægle** (0·0489). The widest chasms in the zone of minor planets are thus found to include those distances at which the periodic times would be commensurable with that of *Jupiter*. These coincidences are not accidental. The *primitive* ring undoubtedly contained nebulous matter at these as well as the intervening distances; and the present existence of such intervals may be accounted for as follows:—

A planetary particle at the distance 2·5 — in the interval between *Thetis* and *Hestia*—would make precisely three revolutions while *Jupiter* completes one; coming always into conjunction with that planet in the same parts of its path. Consequently, its orbit would become more and more eccentric until the particle would unite with others, either interior or exterior, thus forming the nucleus of an asteroid. Even should the disturbed body not

* We here except the most remote interval, that between *Cybele* and *Sylvia*, which is 0·0722.

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come in contact with other matter, the action of *Jupiter* would ultimately change its mean distance, and thus destroy the commensurability of the periodic times. In either case the primitive orbit of the particle would be left destitute of matter. The same reasoning is, of course, applicable to other intervals.

The Rings of Saturn.

The equatoreal radius of *Saturn*, and the dimensions of the two bright rings, according to the measures of Struve,* De La Rue,† Main,‡ and Jacob,§ together with the mean of these measures, are as follows:—

	Struve.	De La Rue.	Main.	Jacob.	Mean.	In Equatoreal Rad. of Primary.
	″	″	″	″	″	″
Equatoreal Radius of Planet ..	8·9955	8·83	8·78	8·95	8·8864	
Exterior Radius of Outer Ring	20·0475	19·915	19·865	19·995	19·9556	2·2456
Interior ,, ,,	17·6445	17·665	..	17·910	17·7398	1·9963
Exterior Radius of Inner Ring	17·2375	16·725	..	17·425	17·1292	1·9276
Interior ,, ,,	13·334	13·455	13·825	13·135	13·4372	1·5121

	h	m	s
The period a Satellite revolving at the distance 1·9276, the interior limit of the interval, is	10	52	11
One-sixth of the period of Dione is	10	56	53
One-third ,, Enceladus	10	59	22
One-half ,, Mimas	11	18	32
One-fourth ,, Tethys	11	19	36
And the period of a Satellite at the distance 1·9963, the exterior limit of the interval, is	11	35	18

It is thus seen that the interval occupies precisely the space in which the periods of satellites would be commensurable with those of the four members of the system immediately exterior. As, therefore, the powerful attraction of *Jupiter* produces the observed *gaps* in the asteroid zone, so *the disturbing influence of Saturn's interior satellites is the physical cause of the permanent interval between the two bright rings.*

These facts and considerations are suggestive in regard to the numerous instances of approximate commensurability in the planetary periods. Let us suppose, in the first place, that at a former epoch the two innermost satellites of *Saturn*, *Mimas*, and *Enceladus*, constituted a broad, flat, and nearly continuous nebular ring, the separate portions of which revolved in different periods;

* *Mem. Ast. Soc.* vol. iii. p. 301.
† *Monthly Notices*, vol. xvi. p. 43.
‡ Ib. p. 35. § Ib. p. 124.

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the matter of the exterior satellites having been previously collected into planetary forms. The first four satellites have the following periods:—

	d	h	m	s
Mimas	0	22	37	28
Enceladus	1	8	53	7
Tethys	1	21	18	26
Dione	2	17	41	9

The ring, therefore, from which the two innermost satellites were (hypothetically) derived must have contained (1) the orbit of a satellite whose period would be $22^h\ 39^m\ 13^s$, or one half that of *Tethys*; (2), that of a body revolving in $21^h\ 53^m\ 43^s$, or one third that of *Dione*; (3), that corresponding to a period of $1^d\ 8^h\ 50^m\ 34^s$, or one-half that of *Dione*; and (4), that of a mass whose period would be $1^d\ 6^h\ 12^m\ 17^s$, or two-thirds that of *Tethys*. The portions of this annulus at the distances named would be *unstable*, like the original matter in the interval of the present ring. The first two distances are nearly the same with that of *Mimas*; the third and fourth with that of *Enceladus*. The collision of masses and the formation of nuclei near the present orbits of these satellites would, therefore, result from the disturbing influence of *Tethys* and *Dione*. The remoter satellites afford similar instances illustrative of the same idea. Still others might be specified, but they will readily occur to every astronomer.

The degree of eccentricity attained by the orbit of such matter previous to its union, near one of the apsides, with other portions of the zone, would probably depend on the density of the original nebulous ring. This, in the case of the ring between *Mars* and *Jupiter*, is known to have been very small; and, accordingly, we find here the greatest excentricities. It is obvious, moreover, that the nearest approximations to commensurability would generally be found in cases where the density of the primitive rings was the greatest.

The instances of nearly commensurable periods in the Jovian and Saturnian systems, as well as among the primary planets, have long been well known, and are too numerous and remarkable to be accidental coincidences. That the physical cause of such phenomena is a legitimate object of research will hardly be called in question. Hitherto, however, no explanation of the facts has been even attempted.

The foregoing view of the Nebular Hypothesis may not be necessary to account for the transmutation of rings into the planetary form. It assigns, however, an obvious cause for the establishment of nuclei in such positions that their periods will be nearly commensurable with that of the disturbing body. As these nuclei would receive accretions of matter from portions of space both interior and exterior to their respective orbits, their

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distances from the central body, during their planetary growth, would not be liable to great variation.

On Aboul Hhassan's Catalogue of 240 *Stars.*

By E. J. Stone, Esq.

The interest attaching to our few ancient Catalogues of stars, is, perhaps, sufficiently great to excuse my calling the attention of the Society to a Catalogue of 240 stars, contained in the work of Aboul Hhassan Ali, translated from the Arabic by J. J. Sédillot. Paris, 1834.

The longitudes and latitudes are given for the commencement of the Hegira, July 15, A. D. 622. It is not known, I believe, from what sources these observations were drawn. Delambre, *Astronomie du Moyen Age*, p. 186, merely says, "Il nous donne un catalogue de 240 étoiles, pour le commencement de l'hégire, c'est-à-dire pour le 15 Juillet, 622. Nous l'avons réduit à l'époque actuelle; il nous a paru trop inexact pour être reproduit ici; les erreurs les plus ordinaires passent un degré. On y voit quelques étoiles australes que ne sont point dans le catalogue de Ptolémie." And, again, on the same page,—"Il donne les déclinaisons de 180 étoiles, pour le commencement de l'hégire, sans nous dire comment ces déclinaisons ont été observées. On peut croire qu'elles sont tirées des ouvrages d'Arzachel."

I find more than 200 of these stars in Ptolemy's Catalogue. I have diminished all the longitudes of Hhassan's Catalogue by 6° 40′, and then compared the latitudes and diminished longitudes with the corresponding latitudes and longitudes of Ptolemy. The agreement is very close. The latitudes are identical; except in a very few cases indeed, and in nearly all these cases of apparent exception, one at least of the different readings given by Baily will be found to agree with the latitude of Hhassan's Catalogue. Ptolemy's longitudes are only given to the nearest 10′.

In some cases Hhassan's longitudes, as given by Sédillot, end with a 7′, 8′, or 9′. In these cases we have respectively a discordance of 3′, 2′, or 1′ between Hhassan's reduced longitude and that of Ptolemy. In nearly every other case we have identical results. The agreement, moreover, between the two Catalogues is such that in three or four cases where Ptolemy's longitudes are evidently 2° or 3° in error, relatively to the other stars, the same error is reproduced in Hhassan's Catalogue. I may mention that in the greater number of cases of discordance between the different versions of Ptolemy quoted by Baily, Hhassan agrees with the copy marked L, "Venice edition, in Latin, by Liechtenstein, 1515," which is known to have been derived from Arabic sources. I am unable to account, in a manner perfectly satisfactory to myself, for the appearance of the slight discordances of 1′, 2′, or 3′,

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of which the discordance of 2′ is by far the most common. These discordances are far too small to express the errors of observations of two independent catalogues. This will appear at once from a comparison between Ptolemy's Catalogue and that of Ulugh Beigh.

It has occurred to me that Ptolemy's places may have been laid down upon a globe and subsequently reproduced, for, perhaps, another epoch, without attention to the restriction of always taking the longitudes to the nearest 10′. This would give rise to small discordances such as really exist between the catalogues. Whatever may be the true explanation of these small differences, I have not the slightest doubt about Hhassan's Catalogue being mainly a reproduction of that of Ptolemy.

There is one point connected with this Catalogue which appears to me to throw great light upon the origin of the belief which existed amongst many of the Arabic astronomers, of an oscillation in the precessional motion. Hhassan gives a Table for the calculation of the precessional motion in longitude in any time. His Table is founded upon the assumption:

$$\text{Precessional motion} = \text{Constant} \times \sin(5'{\cdot}409\, t + 3^\circ{\cdot}51')$$

where t is the number of Arabic years from the commencement of the Hegira.

Hhassan's Table has been computed with natural sines to only two places of decimals, and these not always correct in the second place. It is therefore rather difficult to see the exact value of the constant which has virtually been employed. If, however, we take 610″ for the value, the results agree closely with those given by Hhassan.

Hhassan quotes an observation of Hipparchus and another of Arzachel to show the accuracy of his result. He does not mention Ptolemy's Catalogue.

If we compute from the formula the precessional motion from the commencement of the Hegira to the epochs of the Catalogues of Arzachel, Ptolemy, and Hipparchus, we have

For Arzachel's Catalogue	$t =$	+ 473
Ptolemy's do.	$t =$	− 500
Hipparchus's do.	$t =$	− 775

Hence from the formula

		° ′
Precessional motion to Arzachel's time	=	+ 7 22
Ptolemy's do.	=	− 6 42
Hipparchus's do.	=	− 9 17

Hence between Hipparchus and Arzachel we have a motion of 1.6° 39′. Between Hipparchus and Ptolemy it is 2° 35′. Assuming the uniform precession equal to 50″, we have for the increase of

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longitude between Hipparchus and Arzachel, 16° 45′, agreeing very closely with that given by the formula. The precessional motion between Hipparchus and Ptolemy agrees with that assumed by Ptolemy, viz., 2° 40′. Hence the origin of Hhassan's formula appears to me clear: he could not reconcile the three great catalogues of Hipparchus, Ptolemy, and Arzachel, on the assumption of an uniform change of longitude. He adopted a simple formula which would reconcile these three catalogues, and, not doubting the accuracy of the observations, assumed that this formula must express a true law of nature.

If this view be correct, his belief in the oscillatory motion of the first point of *Aries* arose entirely from the systematic error—of about a degree—in Ptolemy's longitudes; and Ptolemy's want of candour respecting the nature of his Catalogue, is thus found to be throwing astronomy into complexities more than 1100 years after his death.

Measures of ζ Herculis. By G. Knott.

During the past summer I obtained the following measures of the binary star *ζ Herculis* with my 7½-in. Alvan Clark refractor, and a parallel wire micrometer by Dollond.

P	obs.	w.	D	obs.	w.	Epoch
204°·46	5	23	0″·951	4	8	1868·398
206·95	5	23	1·088	4	16	·452
208·19	6	33	1·025	4	22	·474
205·90	5	27	0·896	4	20	·606

Mean results, allowing weight,—

P	obs.	w.	D	obs.	w.	Epoch
206°·53	21	106	0″·992	16	66	1868·48

Considerable pains were bestowed on the distance measures from the fact that with the full aperture of 7·33 inches the image of the small star was projected almost centrally upon (perhaps very slightly *within*) the first bright ring surrounding the primary, thus affording a favourable opportunity for determining the radius of the first ring with that aperture. The result is in most close accordance with that given by theory. The radius of the first bright ring for a telescopic aperture of 7½-in. calculated from the numbers given by the Astronomer Royal on p. 80 of his *Undulatory Theory of Optics*, being 1″·009, which differs by only 0″·017 from the mean result of my own measures.

Woodcroft Observatory, Cuckfield, Sussex,
December 10, 1868.

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Occultations of Stars by the Moon, observed at the Durham Observatory, 1867, November, 1868, May. By J. L. Plummer.

(*Communicated by Prof. Chevallier.*)

Day of Obs.	Phenomena.	Moon's Limb.	G.M.T. h m s
1867. Nov. 6	Disapp. of λ Aquarii	Dark	10 24 57·5
	Reapp. of λ Aquarii	Bright	11 33 52·2
	Disapp. of 78 Aquarii	Dark	11 57 18·6
Nov. 8	Disapp. of 10 Ceti	Dark	7 10 6·1
	(*a*) Reapp. of 10 Ceti	Bright	8 23 8·1
Dec. 11	(*b*) Disapp. of 130 Tauri	Bright	8 47 38·9
	(*c*) Reapp. of 130 Tauri	Dark	9 33 24·3
Dec. 16	Disapp. of *c* Leonis	Bright	13 54 23·9
	Reapp. of *c* Leonis	Dark	15 1 56·8
1868. Feb. 4	Disapp. of 130 Tauri	Dark	6 55 8·6
Mar. 28	(*d*) Disapp. of γ Tauri	Dark	9 1 4·9
	Reapp. of γ Tauri	Bright	9 56 3·6
Apr. 9	(*e*) Disapp. of η Libræ	Bright	13 30 12·7
	(*f*) Reapp. of η Libræ	Dark	14 6 15·3
May 4	Disapp. of σ^2 Virginis	Dark	9 6 45·9
	Reapp. of σ^2 Virginis	Bright	10 17 42·5

(*a.*) Not exact; the Moon's limb unsteady. (*b.*) Not very exact; the star being faint from haze and the Moon only a few hours past the full. (*c.*) Not good; the star faint. (*d.*) The star was attached to the Moon's limb, which was distinctly visible by earthlight, at least 5 seconds before the disappearance, which was instantaneous. (*e.*) The star faint; the observation not very satisfactory. (*f.*) Light clouds passing, but the observation pretty good. (*g.*) Uncertain; the Moon's limb very unsteady.

Durham Observatory.

On a Defective Vision. By Major Tennant, R.E.

Some few members of the Royal Astronomical Society may be aware that I have occasionally complained of a somewhat defective vision. If I venture to enter into some details about it, it is not that the matter is of much importance as regards myself, but that I think many more are labouring under slight defects, and being unconscious of them, attach, in perfect good faith, a value to delicate observations which they do not deserve.

Many years ago, soon after I came to India, I found in look-

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ing at the Snowy Ranges of the Himmaleh with a telescope that to my eye the snow had a reddish-brown tint. Shortly after I found that this was not the same for both eyes, but that, in passing from my right eye to my left with a telescope, I seemed to pass, as it were, from the brownish tints of autumn to the brilliant ones of spring. Every object became brighter and clearer, and the greens were especially affected. Close examination showed me that the vertical and horizontal wires in a telecope were never pleasantly seen together. One was always clearer than the other, and any occasional perfect vision passed away after the effort to see wore off.

I came to the conclusion then that there was really some defect in my eye. In 1857 and 1858 I was examining many of the double stars in Smyth's *Cycle*, and it struck me that I could not see blue stars two magnitudes larger than others there described which I did see. I also noticed that the very red stars described had never produced on me any marked impression of colour, and I deduced that my eyes were really somewhat insensitive to the blue rays.

So matters stood till last December, when I accidentally saw an optometer at Messrs. Horne and Thornthwaite's, which led me to ask Mr. Acland of that firm to examine my eyes. He came to the conclusion that I was somewhat short-sighted, and that the lenses of the eye did not refract as solids of revolution, but that a plano-cylindrical convex lens of 30 inches radius would nearly adjust my vision. I tried these, and, after finding the proper positions of the axes of the cylinders, found that the definition was enormously improved, and that the left eye was, as I thought, nearly perfect, and the right eye nearly as good as the left originally, but I was firmly convinced for some time that I was *not* short-sighted. At last, however, I became convinced that I was so with these lenses, and a spherical surface of the same radius as the cylinder was ground inside it. Now my vision was superior to anything before. On the voyage out I have often seen eight stars of the *Pleiades* (and notes of their places are in a note-book) where I never before saw more than five; but I found now I had become long-sighted, and could not use the glasses for reading, and became convinced that Mr. Acland's statement was correct, namely, that I was short-sighted, not because the near focus was short, but because I had a want of the usual range of adjustment and could not reach the distant one; when, therefore, distant vision was corrected the near vision became defective. I therefore made up my mind to have two sets of lenses.

But before ordering them I determined to re-examine my eyes with great care, and I now found, on using an achromatic glass as an eye-glass, and a peculiar object, that, in reality, the estimate I had made of the improvement in my right eye was very nearly right. I now found that the convex surface for the right eye required a radius of 20·5 inches, and for the left eye

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one of 40, and that the concavity for distant vision required to be 27·5 inches in radius for each.

The red tint now remained to be accounted for, and I have been led to what I believe to be the true solution of that, and also of the insensibility of the right eye to blue light. I should mention that habit makes me see mainly with the right eye, that is, that its image clearly predominates in ordinary vision. I had noticed that while my general vision of stars was much improved with the spectacles I had brought from England, and the sky became dotted with small objects invisible without them, some stars certainly were less clear. It only occurred to me recently that on that occasion *α Scorpii* was injured and *Saturn* improved in definition by the glasses, and I then remembered that *α Scorpii* had generally been the defaulter, the extreme beauty of that constellation having always made it catch my eye. It is evident *now* that my right eye is not achromatic, that the red rays being naturally focussed predominate in vision, and that when the luminous image of a star is focussed the blue rays do not claim notice, but when a blue star is seen, if it be so small as to be near the minimum of brightness, its image is too diffused to be visible.

Now, my vision I used to think good. I have often at sea caught the masts and rigging of a ship before those searching for them, and the defects which I have spoken of I have frequently believed to be fancies, as they seemed inconsistent with many evidences that I had good vision. I should certainly have unhesitatingly trusted my eyes in any matter but colour. Possibly some of the strange discrepancies in description are due to similar defects of vision, and we may, some day, find it necessary to make every observer of delicate phenomena, who claims credit, *prove that he can see.*

Calcutta, Oct. 15, 1868.

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Printed by STRANGEWAYS AND WALDEN, Castle Street, Leicester Square, and Published at the Apartments of the Society, February 4, 1869.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIX. *February* 12, 1869. No. 4.

Admiral MANNERS, President, in the Chair.

W. J. H. Beechey, Esq., Westbury-on-Severn;
Jas. Robert Jones, Esq., Aberdeen;
John Lee, Esq., St, Peter's Chambers, Cornhill;
Dr. Geo. Royston Pigott, 2 Lansdowne Crescent, Notting Hill; and
T. Ryle, Esq., Eastbourne,

were balloted for and duly elected Fellows of the Society.

Report of the Council to the Forty-ninth General Meeting of the Society.

Progress and present state of the Society:—

	Compounders.	Annual Contributors.	Non-residents.	Patroness, and Honorary.	Total Fellows.	Associates.	Grand Total.
December 31, 1867	179	286	13	4	482	46	528
Since elected ...	5	18	...	...	...	...	...
Deceased	−5	−5	−1	−1	...	−1	...
Removals	+3	−3	...	...	...	...	...
Dec. 31, 1868 ...	182	296	12	3	493	45	538

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Mr. Whitbread's Account as Treasurer of the Royal Astronomical Society, from January 1 to December 31, 1868:—

RECEIPTS.

	£	s.	d.	£	s.	d.
Balance of last year's account	...		...	117	18	9
By Dividend on £2800 Consols	40	19	0			
By ditto on £5000 New 3 per Cents ...	73	2	6			
By ditto on £2800 Consols	40	19	0			
By ditto on £5000 New 3 per Cents ...	73	2	6			
				228	3	0
On account of arrears of contributions ...	141	10	0			
182 annual contributions	382	4	0			
22 admission-fees	46	4	0			
17 first years' contributions	26	5	0			
				596	3	0
7 compositions	...		...	144	18	0
Sale of publications:—						
At the Rooms of the Society	79	8	0			
By Messrs. Williams and Norgate, Publishers	42	9	0			
				121	17	0
				£1208	19	9

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EXPENDITURE.

	£ s. d.	£ s. d.
Salaries:—		
Editor of Publications	60 0 0	
Assistant Secretary	130 0 0	
Commission on Collecting	33 16 0	
		223 16 0
Taxes:—		
Land and Assessed	6 11 0	
Income	2 1 8	
Poor Rate	4 15 10	
Other Parish Rates	2 18 4	
		16 6 10
Bills:—		
Strangeways and Walden, printers ...	173 4 0	
Rumfitt, bookbinder	30 3 2	
Basire, engraver	16 7 6	
Malby, lithographer	5 5 0	
Harrild, ,, ...	3 10 0	
Cooke and Sons, instruments	13 13 0	
Insurance	10 0 6	
		252 3 2
Miscellaneous items:—		
House expenses	26 8 3	
Postages	46 8 11	
Books and parcels	7 11 10	
Expenses of evening meetings	13 13 0	
Waiters attending meetings	3 17 0	
Coals and wood	12 0 0	
Gas	6 10 0	
Repairs	1 9 8	
Sundries	9 6 0	
		127 4 8
		619 10 8
Turnor Fund	1 13 6	
Mrs. Jackson's annuity, 1 year	8 15 6	
		10 9 0
Investment:—		
Purchase of £200 Consols...		188 5 0
Banker's Commission on Country Cheque		0 0 6
		818 5 2
Balance at Banker's		390 14 3
		1208 19 5
Add error of 4*d.* in the Petty Cash Account, carried to Petty Cash Account for 1869		0 0 4
		£1208 19 9

F. C. Penrose,
H. Perigal,
J. H. Dallmeyer,
} *Auditors.*

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Assets and Present Property of the Society, January 1, 1869:—

	£	s.	d.	£	s.	d.
Balance at Banker's	...		...	390	14	3
2 Contributions of 7 years' standing ...	29	8	0			
9 ,, 5 ,, ...	94	10	0			
16 ,, 4 ,, ...	134	8	0			
10 ,, 3 ,, ...	63	0	0			
23 ,, 2 ,, ...	96	12	0			
36 ,, 1 ,, ...	75	12	0			
Balance of an Account...	5	10	0			
2 admission-fees, and first contributions ...	6	6	0			
				505	6	0
Due for Publications				2	14	6
£5000 New 3 Per Cents (including Mrs. Jackson's Gift, £300).						
£3000 Consols, including the Lee Fund (£100) and Turnor Fund (£500).						
Unsold Publications of the Society.						
Various astronomical instruments, books, prints, &c.						
Balance of Turnor Fund (included in Treasurer's Account)				125	18	0

Stock of volumes of the *Memoirs*:—

Vol.	Total.	Vol.	Total.	Vol.	Total.
I. Part 1	13	XI.	188	XXIV.	189
I. Part 2	53	XII.	194	XXV.	199
II. Part 1	71	XIII.	205	XXVI.	206
II. Part 2	35	XIV.	394	XXVII.	460
III. Part 1	88	XV.	177	XXVIII.	421
III. Part 2	107	XVI.	203	XXIX.	452
IV. Part 1	107	XVII.	185	XXX.	201
IV. Part 2	118	XVIII.	181	XXXI.	177
V.	128	XIX.	191	XXXII.	208
VI.	154	XX.	186	XXXIII.	215
VII.	178	XXI. Part 1	216	XXXIV.	213
VIII.	164	XXI. Part 2	100	XXXV.	185
IX.	167	XXI. (together).	94	XXXVI. (with M. N.)	267
X.	179	XXII.	185	XXXVI. (without)	68
		XXIII.	182		

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The instruments belonging to the Society are as follows:—

The *Harrison* clock,
The *Owen* portable circle,
The *Beaufoy* circle,
The *Beaufoy* transit,
The *Herschelian* 7-foot telescope,
The *Greig* universal instrument,
The *Smeaton* equatorial,
The *Cavendish* apparatus,
The 7-foot Gregorian telescope (late Mr. Shearman's),
The Variation transit (late Mr. Shearman's),
The Universal quadrant by Abraham Sharp,
The *Fuller* theodolite,
The Standard scale,
The *Beaufoy* clock, No. 1,
The *Beaufoy* clock, No. 2,
The *Wollaston* telescope,
The *Lee* circle,
The *Sharpe* reflecting circle,
The *Brisbane* circle,
The *Short* universal instrument.

The *Sheepshanks'* collection of instruments, viz.,—

1. 30-inch transit, by Simms, with level and two iron stands.

2. 6-inch transit theodolite, with circles divided on silver; reading microscopes, both for altitude and azimuth; cross and siding levels; magnetic needle; plumbline; portable clamping foot and tripod stand.

3. $4\frac{6}{10}$ achromatic telescope, about 5 feet 6 inches focal length; finder, rack motion; double-image micrometer; object-glass micrometer; two other micrometers; one terrestrial and ten astronomical eyepieces, applied by means of two adapters, with equatorial stand and clock movement.

4. $3\frac{1}{4}$-inch achromatic telescope, with equatorial stand; double-image micrometer; one terrestrial and three astronomical eyepieces.

5. $2\frac{3}{4}$-inch achromatic telescope, with stand; one terrestrial and three astronomical eyepieces.

6. $2\frac{3}{4}$ achromatic telescope, about 30 inches focus; one terrestrial and four astronomical eyepieces.

7. 2-foot navy telescope.

8. 45-inch transit instrument, with iron stand, and also Y's for fixing to stone piers; two axis levels.

9. Repeating theodolite, by Ertel, with folding tripod stand.

10. 8-inch pillar-sextant, divided on platinum, with counterpoise stand and horizon roof.

11. Portable zenith instrument, with detached micrometer and eyepiece.

12. 18-inch Borda's repeating circle, by Troughton.

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13. 8-inch vertical repeating circle, with diagonal telescope, by Troughton and Simms.

14. A set of surveying instruments, consisting of a 12-inch theodolite for horizontal angles only, with extra pair of parallel plates; tripod staff; in which the telescope tube is packed; repeating table; level collimator, with micrometer eyepiece; and Troughton's levelling staff.

15. Level collimator, plain diaphragm.

16. 10-inch reflecting circle, by Troughton, with counterpoise stand; artificial horizon, with metallic roof; two tripod stands, one with table for artificial horizon.

17. Hassler's reflecting circle, by Troughton, with counterpoise stand.

18. 6-inch reflecting circle, by Troughton, with two counterpoise stands, one with artificial horizon.

19. 5-inch reflecting circle, by Lenoir

20. Reflecting circle, by Jecker, of Paris.

21. Box sextant and 3-inch plane artificial horizon.

22. Prismatic compass.

23. Mountain barometer.

24. Prismatic compass.

25. 5-inch compass.

26. Dipping needle.

27. Intensity needle.

28. Ditto ditto.

29. Box of magnetic apparatus.

30. Hassler's reflecting circle, with artificial horizon roof.

31. Box sextant and 2¼-inch glass plane artificial horizon.

32. Plane speculum artificial horizon and stand.

33. 2½-inch circular level horizon, by Dollond.

34. Artificial horizon roof and trough.

35. Set of drawing instruments, consisting of 6-inch circular protractor; common ditto; 2-foot plotting scale; two beam compasses and small T square.

36. A pentagraph.

37. A noddy.

38. A small Galilean telescope, with the object lens of rock-crystal.

39. Six levels, various.

40. 18-inch celestial globe.

41. Varley stand for telescope.

42. Thermometer.

43. Telescope, with the object-glass of rock crystal.

These are now in the apartments of the Society, with the exception of the following, which are lent, during the pleasure of the Council, to the several parties under mentioned, viz.:—

The *Fuller* theodolite, to the Director of the Sydney Observatory.

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The *Beaufoy* transit, to the Observatory, Kingston, Canada.

The *Sheepshanks* instrument, No. 1, to Mr. Lassell.
Ditto ditto No. 3, to Major Tennant.
Ditto ditto No. 4, to Rev. C. Lowndes.
Ditto ditto No. 5, to Mr. Birt.
Ditto ditto No. 6, to Rev. J. Cape.
Ditto ditto No. 8, to Rev. C. Pritchard.
Ditto ditto No. 9, to the Director of the Sydney Observatory.
Ditto ditto No. 41, to Rev. C. Pritchard.
Ditto ditto No. 43, to Mr. Huggins.
The 6-inch circular protractor, to Mr. Birt.

The Gold Medal.

The Council have awarded the Gold Medal to Mr. E. J. Stone for his Rediscussion of the Observations of the Transit of *Venus* in 1769, and his other contributions to Astronomy. The President will, in the usual way, explain the grounds of this award.

Memoirs.

The next volume of *Memoirs* will contain Major Tennant's paper on the Solar Eclipse of August 18, 1868. The paper will be accompanied by engravings of the photographs, enlarged under the superintendence of Mr. Warren De La Rue.

Index to the Publications of the Society.

An index to all the papers contained in the publications of the Society, from its commencement down to the end of 1868, is nearly finished, and will shortly be forwarded to press. The index will contain all the papers contributed by each author under his name, and also a list of papers contributed under the head of each subject.

Obituary.

The Society has to regret the loss by death of the following Fellows and Associates:—

Fellows.—Philip James Chabot.
Sir David Brewster.
Rev. W. R. Dawes.
Thomas Cooke.
H. E. Westcar.

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Major-General Shortrede.
Robert Porrett.
W. S. Downes.
John Jenkins.
W. H. Barton.
Robert Worthington.

Honorary Fellow.—The King of Siam.

Associate.—Leon Foucault.

The late Rev. WILLIAM RUTTER DAWES, Fellow of this Society, was born on the 19th of March, 1799, at Christ's Hospital, where his father was Mathematical Master. He lost his mother while very young, and, his father having been appointed Governor of Sierra Leone, he was placed under the care of his grandfather at Portsmouth. In 1807 he was transferred to the family of the Rev. Thomas Scott, the revered Commentator on the Bible, where he remained till 1811, in which year, on his father's return to England, he was placed at the Charter House. In 1813, however, on his father's again leaving England for the West Indies, he was removed thence and once more placed under the care of the Rev. Mr. Scott, at Aston Sandford, where he remained until the death of that excellent man, who breathed his last in his pupil's arms.

Designed by his father for a clergyman in the Church of England, he studied under Mr. Scott with that object in view. A mind like his, however, could not receive dogmas, however consecrated by general approval, without giving them a searching examination. In his case the result was, that he became dissatisfied as to some at least of the tenets of the Established Church, specially relating to ecclesiastical government. Instead, however, of rushing headlong in the opposite direction, young Mr. Dawes turned his thoughts to the study of Medicine, for which he was eminently qualified by nature. Having gone through the usual course at St. Bartholomew's Hospital, under Abernethy and the late Sir Wm. Lawrence, he returned to the neighbourhood which in Mr. Scott's society he had learned to love, and settled as a medical practitioner at Haddenham, Berks. Here he married his first wife, the widow of his late tutor and friend, who, though much older than himself, contributed greatly to his happiness by her vivacity and Christian gentleness.

Domestic arrangements arising out of the death of an only and very dear sister caused him, however, to give up his practice at Haddenham, and brought him to Liverpool in the year 1826. Here his former desire to take holy orders seemed to revive; but he was unable to escape from the scruples which had formerly deterred him from so doing in the Church of England. While in this unsettled state of mind, Mr. Dawes was brought into contact with the late Dr. Raffles, Minister of the leading Independent Congregation in Liverpool. A man possessing such gentlemanly

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feelings, and acting on such enlarged and Christian principles, at once exercised a powerful influence over Mr. Dawes, and he was at length induced to take charge of a small congregation of the Independent denomination at Ormskirk. The name of this place is the first which is associated with him in that scientific career which he afterwards adorned. Partial to Astronomy from a youth (for he has left sketches of his observations while with Mr. Scott — a taste inherited from his father) — Mr. Dawes continued his observations while in Liverpool with a small telescope (of less than 2 inches aperture) through an open window.*

At Ormskirk, however, he erected his first Observatory; on a small scale, the principal instrument being a 5-foot refractor by Dollond, aperture 3·8 inches; and there became known as an earnest and precise observer. On the 14th May, 1830, he was elected a Fellow of the Royal Astronomical Society, and, his health having some time previously failed, he was compelled to resign his ministerial charge, and in the early autumn of 1839† he took charge of Mr. Bishop's Observatory at South Villa, Regent's Park; himself residing in St. John's Wood. While here, it was that he married a second time (his former wife having died at Ormskirk some years previously). This happy and judicious union with the widow of John Welsby, Esq., Solicitor, of Ormskirk, which occurred in 1842, contributed materially to the comfort and usefulness of his future life. Soon after, on the termination of his connexion with Mr. Bishop in the spring of 1844, Mr. Dawes removed to Cranbrook, in Kent, to a residence called Camden Lodge, where he erected the Observatory described in the sixteenth volume of the *Transactions* of this Society, which, in 1846, he enriched with a fine equatoreal telescope of 8½ feet focus and 6 inches aperture, by Merz, of Munich, admirably adapted by a most delicate clock-work movement for his observation and measurement of double-stars, and the satellites of *Saturn* and *Uranus*. With this he continued indefatigably to observe, suffering, however, deplorably from continual and most distressing headaches and other symptoms of increasing ill-health, which, at length, obliged him to resort to a residence at Torquay, and had

* This was probably the telescope referred to in the following extract from one of his letters, dated 17 Dec. 1867, addressed to Sir J. Herschel: —

"Having obtained the loan of a volume of Rees's *Encyclopædia*, I had copied out the list it contained of Sir Wm. Herschel's Catalogues of Double-Stars, arranged in classes and constellations; and with a capital little refractor of only 1·6 inches aperture, and a copy of the French edition of Flamsteed's Atlas, which was presented by Dr. Maskelyne to my father before he went out in 1787 as Government Astronomer to the first expedition to Botany Bay under Governor Philip. I worked away on almost every fine night, when uncertain health would permit, and found and distinctly made out a large number of the objects in the second and third classes, among which were *Castor*, *Rigel*, ι^1 and ι^2 *Lyræ*, σ *Orionis*, ζ *Aquarii*, and many others, of which I made correct diagrams in a book now lying before me, as well as of γ *Virginis* (then in the same quadrant that it is now), *Polaris*, &c. &c. The difficulty often was to get to bed in summer before the Sun extinguished the sight of the game."

† Some time between Aug. 10 and Oct. 1.

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nearly decided him to dispose of his instruments and give up observing. A favourable change, however, took place, and in 1850 he removed his observatory to Wateringbury, near Maidstone, and finally, in 1857, to Hopefield, Haddenham, near Thame, where he continued to reside till his decease. Here it was that the accurate observations and ripened experience of many years bore their fruit in the universal esteem with which they were regarded, as well as in the volume completed and printed just before his death by the Astronomical Society, under the title of "Catalogue of Micrometrical Measures of Double Stars," forming part of the thirty-fifth volume of their *Transactions*. This work, taken in conjunction with the earlier series of measures of 121 double-stars, contained in vol. viii. of the *Mem. R. Ast. Soc.*, and of his Ormskirk Measures, communicated to the Astronomical Society in 1849–50, and forming part of the nineteenth volume of their *Memoirs*, and with those made with Mr. Bishop's 7-inch refractor from 1839 to 1844, published by that gentleman in a separate volume of observations at South Villa, form a monument of unwearied industry and minute and painstaking precision, to which it would be difficult to find a parallel in the record of private and unassisted observation. The instrumental means at his disposal in the interval from 1854 to 1865 were augmented by the purchase in 1854 of an admirable 7½-inch refractor by Alvan Clarke, which was replaced in May 1859 by a still larger instrument by the same maker of 8¼ inches clear aperture and 110 inches focus, equatoreally mounted, at Haddenham, as described in the *Ast. Soc. Notices*, vol. xx. p. 60. Many of his measures, too, commencing with April 13, 1865, were made with an 8-inch refractor by Messrs. Cooke and Son, so that nothing which instrumental means could supply is wanting to render these later observations landmarks of ultimate and invaluable reference in this department of Astronomy.

In the year 1855 Mr. Dawes was presented with the Gold Medal of the Royal Astronomical Society. In his address to the Society on that occasion, the Astronomer Royal (the President for the year), while laying all due stress on the excellence of Mr. Dawes's measurements of double-stars from 1831 to 1844, very properly calls attention to his *independent discovery* in (Nov. 25 and 29) 1850 of the interior dusky ring of *Saturn*, noticed by Prof. Bond on the other side of the Atlantic, on Nov. 15th of the same year, of which observation the first account reached England on Dec. 3d. The astonishing phenomena of the total Solar Eclipse of 1851, too, as he states in the same address, were observed by Mr. Dawes, who made the voyage to Sweden for that purpose.

In 1865 Mr. Dawes was elected a Fellow of the Royal Society.

Endowed with great perseverance and schooled to great accuracy of observation and description, Mr. Dawes's contributions to astronomical science will ever be regarded as of high value.

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He possessed also great ingenuity, and was constantly experimenting. His solar eye-piece, which, by stopping out the blinding light of the photosphere, allows us to obtain a distinct and prolonged view of the interior or nuclear portion of the spots, has afforded us as its practical result, the knowledge of the existence of an interior envelope of the Sun's body which had never before been suspected.

Devoted as he was to Astronomy, the subject of this brief notice was ever ready to impart gratuitous advice to the sick, and will be remembered as their benefactor for years to come by the families of the poor inhabitants of Haddenham; and while his name is known and revered among the learned of different nations and remembered with affection by his relations and friends, it will ever be cherished with grateful feelings by the many recipients of his seasonable medical advice and treatment.

After the loss of his second wife, which occurred in December 1860, his health, always feeble, as a natural consequence of the deep depression of his spirits, gradually gave way. In addition to his habitual and distressing headaches, and attacks of nervous and spasmodic asthma, a heart affection manifested itself, rendering perfect stillness for hours together, and avoidance of the slightest excitement, necessary.

In the intervals of comparative ease accorded him from these attacks, however, he was able to continue his observations, the results of which up to the middle of 1865 (so far as the measurements of Double Stars are concerned)are given in an Appendix to the great Catalogue already spoken of. Much of his attention, too, was given to the discussion as to the nature of that peculiar mottling of the solar photosphere which has been compared to "willow-leaves" and to "rice-grains," but in which he could never be brought to allow that kind of generic form and magnitude which would go to stamp them as "entities" of some special kind modelled according to some definite type: likening them (while not denying their occasional and even frequent visibility as bodies having *size* and distinguishable *shape*) rather to bits of straw or irregular flocculi; nor can this persistent testimony, coming from an observer of such singular acuteness of vision and of such unwearied patience in scrutiny, be lightly cast aside.

Two years before his death Mr. Dawes had a serious attack of illness, from which he so far rallied that his friends hoped his valuable life might yet have been spared many years. Some weeks before his last attack he had been ailing, and seemed prepared for something serious. Up to the last he retained the entire possession of his faculties and his interest in his astronomical pursuits. Only twenty-four hours previous to his decease he had his "Measurements" on his knee in bed, explaining something there to his medical attendant.

Combined with an ardent, inquiring, and mathematically reasoning mind, Mr. Dawes possessed an implicit and childlike faith in the great doctrines of Revelation, and, though ready to dispute

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many of the accepted theories of theology, never for a moment let go the cardinal doctrine of salvation by faith only in our Lord Jesus Christ. After some hours of unconsciousness, he peacefully breathed his last on Feb. 15, 1868, and lies interred beside his dear wife in the churchyard of Haddenham.*

The late Major-General ROBERT SHORTREDE was born at Jedburgh, in Roxburghshire, on the 19th July, 1800, and was the son of Mr. Robert Shortrede, Procurator-Fiscal of that county. He was educated first at the Jedburgh Grammar School, and afterwards at the Edinburgh High School, whence he went to Edinburgh University, where he attended the Mathematical and Natural Philosophy classes for four years, and attained considerable distinction. During that time he was also in the office of Mr. Robert Stevenson, the engineer of the Northern Lighthouses, as he was originally intended for a civil engineer, having early evinced unusual aptitude for Mathematics; but his friends, thinking that India presented ample scope for his talents in that direction, he obtained an appointment to that country from the late Mr. Canning, through the influence of Sir Walter Scott, an old and intimate friend of his family; but unfortunately he was then too old to go to Addiscombe, and so into the Engineers in the usual course, and was therefore obliged to enter the Infantry of the line, which he afterwards found to be a serious hindrance to his scientific advancement. Shortly after his arrival in India in 1822, he was appointed to the Deccan Survey, and was soon afterwards employed to conduct a triangulation over the Bombay Presidency, in the course of which he measured a base-line on the Karleh Plain, which when connected with that from Seronj by the Bombay Longitudinal Series, has proved to be not inferior in correctness to bases measured in the Great Arc. In 1834 at the request of Lord Clare's government, he was appointed to examine and report upon an extensive Revenue Survey and Assessment of the Deccan, in the course of which it was believed that great frauds had been committed. For the manner in which this duty was performed he received the thanks of Government, and shortly afterwards, without solicitation on his part, was put in civil charge of a district which had long been in disorder, with almost discretionary power, to revise the rates, and put it into order generally. He lowered the assessment and increased the revenue, and in the course of two years brought 40,000 acres of land into cultivation on moderately increasing leases. While so employed he had a dangerous illness, and before the end of his sick-leave, on the sudden death of the Mint Engineer, he was employed for a few months as assistant in the Mint until some permanent arrangement could be made. Shortly afterwards Captain Jacob, then Astronomer at Madras, went on sick-leave to the Cape, and Captain Shortrede was put in charge of the work till

* The leading biographical features of this notice were communicated by, and are given in great measure in the words of, a near relative of the deceased.

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his return. He was afterwards permanently appointed to the Great Trigonometrical Survey, in which he remained till 1845, when he came home home on sick-leave, and while in England drew out a paper on the attraction of the Himalayas as affecting the northern end of the Great Arc, which was shown to and approved of by Lieutenant Raper, Admiral Smyth, Sir J. Lubbock, and Sir J. Herschel, and afterwards submitted to the Royal Astronomical Society, and printed in their *Transactions* for 1845. He also published at this time some Mathematical and Logarithmic Tables, probably the most extensive and correct ever published by a private individual without a subscription list, and which have received the approval of several distinguished mathematicians and astronomers, both British and foreign, amongst others the Astronomer Royal. In 1849, he returned to India, and was appointed to the Revenue Survey in the Punjaub, where he remained till 1856, when he was ordered with his regiment (the Second Bombay Europeans) to Persia, and served through that campaign: he returned to England in 1859. He soon afterwards retired, since which time he published some Traverse Tables, and was engaged at the time of his death in correcting the proofs of some Navigation Tables, compiled with a view to facilitate keeping a reckoning at sea. Throughout his career he was an earnest Christian and philanthropist. He died at Blackheath after a few hours' illness from disease of the heart, on the 26th November, 1868, aged 68.

Mr. JOHN JENKINS was born in London in 1802; while young he accompanied his parents to Swansea, where he subsequently carried on the business of a watch and chronometer-maker,—a vocation which seems to have had considerable influence in determining his pursuits in after years. His contributions to local scientific observation commenced at an early period of his life. His meteorological observations were conducted during a long series of years, and the voluminous tables supplied by him on occasions of more than one official inquiry, attest his unwearied industry, while they obtained for him the admiration and esteem of eminent scientific men engaged in those inquiries. In 1842, Mr. Jenkins accomplished his long-cherished wish of erecting an observatory on his business premises in Swansea. In effecting this object he consulted not only his own tastes for astronomical observation, but the convenience of the numerous foreign trading vessels which about this time began to resort to the port; the observatory was open to all masters and other officers of ships trading to Swansea. It may be remarked that the transit instrument in this observatory was fitted up under the personal superintendence of the late Mr. Sheepshanks, for whom Mr. Jenkins entertained the highest regard, and through whose instrumentality he became a fellow of the Royal Astronomical Society. Here after the business engagements of the day, Mr. Jenkins was for many years in the habit of devoting the hours of night to astronomical observation. In all local efforts for the promotion of the

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interests of science and the diffusion of useful knowledge Mr. Jenkins took an active and often a prominent part. His labours on behalf of the Royal Institution of South Wales were for many years unremitting, and were duly acknowledged by that body on his retirement from official connexion with it some two years since. Referring to organisations of a more popular character for the diffusion of scientific knowledge, so long ago as 1826 we find Mr. Jenkins a prominent promoter of the first attempt to establish a Mechanics' Institution in the town of Swansea, and in all subsequent efforts for the same object he was equally prompt in giving personal services and pecuniary aid to the cause of popular enlightenment, while his sympathies with the social and moral amelioration of the industrial classes are attested by the fact recorded in a local notice of his death that "his patient labours over many years in the promotion of Benefit Building Societies in Swansea and in its neighbourhood give him signal claims to public remembrance." Mr. Jenkins retired from the business which he had so successfully conducted some fifteen years since. For the past three years he had been in declining health from a disease, the sufferings of which he bore with fortitude and equanimity, but under which he finally sank, leaving behind him the cherished recollection of one who, according to his opportunities, had well discharged the "stewardship of life." He died at his residence, Rotherslade, Swansea, on December 16, 1868.

Mr. William Samuel Downes was the eldest son of Mr. James John Downes, whose death was recorded in last year's Report. He evinced in early life great literary and scientific ability, but receiving at the early age of twenty-seven the important and responsible appointment of actuary and secretary to the Law Life Assurance Society, he thenceforth devoted his whole mind to the conduct of the affairs of that important institution. He was one of the founders of the Actuaries' Club, and acted as its treasurer for some time. He died on the 5th of December, 1868, in the forty-ninth year of his age.

Robert Porrett was born in 1782. His father held the office of Ordnance Store-keeper in the Tower, to which office the subject of the present memoir succeeded at his decease. He became a member of the Spitalfields Mathematical Society in 1811. He was an experienced chemist. His first communication in that science was printed in the *Transactions of the Society of Arts*, vol. xxvii. 1809, it is entitled "On Prussic and Prussous Acids." In 1813, in conjunction with Messrs. Wilson and Rupert Kirk, he published an important series of experiments on the explosive compound of chlorine and nitrogen, in vol. xxxiv. of Nicholson's *Journal*. The *Phil. Trans.* for 1814 contain a paper of his "On Triple Prussiates and on Acids formed by the Element of Prussic Acid with Sulphur," and another entitled "On the Constitution of the Prussic, Ferro-chyasic, and Sulpho-chyasic Acids." He gave the name of ferro-chyasic acid to that acid chemists

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now call hydrogen ferro-cyanide, from its being compounded of iron, carbon, hydrogen, and nitrogen. His investigations on endosmose or the elective transmission of liquids through a membranous diaphragm was published in the *Annals of Philosophy*, vol. viii. 1816. This curious property of membranes has since been thoroughly investigated by Prof. Graham, whose researches have ultimately led to a new method of analysis called "dialysis." He contributed many valuable essays to the other scientific journals, the latest in conjunction with the late Mr. Teschmacher was read before the Chemical Society in 1846, it is entitled, "On the Composition of Gun-cotton." Throughout his career as a chemist he was distinguished for the great originality of his views. He was elected a Fellow of the Royal Society in 1848, and was also a Fellow of the Society of Antiquaries and of the Chemical Society. He became a Fellow of this Society on its junction with the Mathematical Society in 1845. In addition to his chemical pursuits he was much devoted to antiquarian researches, particularly as connected with ancient weapons and armour, and was always ready to give any information in his power on such subjects. He was for many years Treasurer of the Chemical Society, and was on more than one occasion pressed to accept the Presidentship, which he declined on the ground of his advanced age, which, however, did not prevent his being a frequent attendant at the meetings of the various scientific Societies with which he was connected. He died on the 25th of November, 1868.

Philip James Chabot was born in Spitalfields in 1801. His father, who was descended from one of the refugees who were obliged to leave their native country in consequence of the Revocation of the Edict of Nantes, was an eminent dyer in that parish. After the usual classical education, Mr. Chabot entered at St. John's College, Cambridge, and took his degree of M.A. in 1827. In 1830 he was called to the Bar, but, on the decease of his father in 1832, he was induced by the earnest entreaties of his friends, although much against his own inclination, to quit the legal profession and engage in his late father's business as a dyer. He subsequently made several improvements in some of the branches of dyeing; but finding the business did not answer his expectations, he retired from it, and became the originator of the Silk Conditioning Society, of which he was Secretary and Manager to the time of his death. He was elected a Member of the Spitalfields Mathematical Society in 1834, and at the junction of that Society with this in 1845, he became a Fellow of this Society. He was a Member of the Cavendish and Philological Societies, and also of the Chemical Society, from which he had retired a few years before his death. He was a well-read and intelligent man, and much interested in science and general literature, and was greatly respected for his probity and urbanity of manners. He died suddenly January 11, 1868.

William Parsons, Earl of Rosse, the eldest son of Lawrence,

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the second Earl, was born at York on the 17th of June, 1800. His mother was Alice, daughter of John Lloyd, Esq., of Gloster, King's County. His preparatory education he received at home, from a private tutor, and in 1818 he entered the University of Dublin; remaining there, however, only till the following year, when he passed to Magdalen College, Oxford. Here he took high honours, receiving his B.A. degree as a First Class in Mathematics, in 1822. While bearing the title of Lord Oxmantown he was elected to represent King's County, and sat in Parliament from 1821 till 1834, when he retired from political life — for a while only, however — to devote himself more closely to the duties of his social position, and to follow with greater freedom the philosophical pursuits into which the bent of his mind had led him. In the meanwhile, in 1831, he had been appointed Lord Lieutenant of his County, and in 1834 he was made Colonel of its Militia. Two years after this he was married to Mary, eldest daughter of the late John Wilmer Field, Esq. of Heaton Hall, Yorkshire: in 1840 the present Earl, the eldest of four sons, was born.

The second Earl died in the year 1841, and the subject of this memoir succeeded to the title; but his Irish coronet giving him no seat in the House of Lords, he was still enabled to pursue without serious interruption the objects of his scientific taste. In 1845, however, he was elected an Irish Representative Peer, and thus he held a seat for life in the House of Lords. Although he occupied it with good regularity, he took little active part in debate: his voice was seldom, if ever, heard during the stormy period which embraced the discussions upon Irish Emancipation and Reform. But if not a talking, he was a working member, an invaluable referee in matters of business, and an assiduous servant upon committees. Throughout his Parliamentary career he gave his support to the Conservative party. Dwelling among an excited people in troublous times, he was firm in enforcing the authority of the law, even to the risk of his own personal safety; but withal he was kind and beneficent, devoting nearly all the rental of his Irish property to the relief of his suffering countrymen, thereby winning for himself a place in their hearts, and in time diverting the hostile feelings with which he had been regarded. His genius was eminently versatile. One among his friends, who had the best opportunities for discerning the extent and varied nature of his abilities, tells us that few minds of our day have grasped so wide a range of knowledge. Of his engineering skill and his mechanical resource he has left us monuments that speak for themselves; but of his mastery of political economy, of his capacity as a chemist, of his acquaintance with military and nautical sciences, he has left us no such memorials, though it was quite in his power to have created them.

Astronomy was the science of his adoption; the manufacture of stupendous telescopes the branch of it to which he devoted undivided attention. It was his aim to extend this to its farthest limit of practicability; to make a telescope of the largest dimensions possible with the resources of his time. The reflecting

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principle was that to which he was compelled to resort. There was no hope of any such immediate advance in the manufacture of optical glass as would supply disks of the size and quality requisite to surpass to any degree the refractors then in use. We are speaking of the date of his first turning attention to the subject, which was about 1826. He made some few experiments upon fluid lenses, but these gave no promise. To perfect the reflector was the only course open to him. But it was one beset with difficulties that could be foreseen, and obstacles that arose only as the work grew under his hand. To start with, the Earl of Rosse had to originate everything; the experiences of his predecessors in the same domain of art were of no avail to him. Short, who had excelled all who went before him in casting and polishing specula, was so jealous of posterity reaping the fruits of his knowledge, that before his death he ordered all his tools to be destroyed. The Earl of Stanhope made great endeavours to attain magnitude in reflecting telescopes, but his attempts were failures. He bequeathed his papers to the Royal Institution, and Sir James South searched them through for memoranda bearing upon the subject, thinking that the failures of such a man might in their way be instructive; but he did not light on a single hint that was worth remembering. Sir William Herschel was the master who, from the majestic scale of his instruments, might have been expected to have given the Earl of Rosse an example to work from; but his methods were a sealed book—what was the composition of his metals and how he worked them to figure and polish have never been made known.

The inaugural difficulty was in the selection of an alloy possessing the qualities of whiteness, brilliancy, and resistance to tarnish requisite for a mirror. After many trials a mixture was decided upon as answering these conditions,—an alloy of four atoms of copper to one of tin. But such a compound, though harder than steel, is more brittle than glass, very unmanageable in the founder's hands, not yielding a sound casting by ordinary processes, and when cast liable to crack in cooling, or afterwards by any sudden change of temperature. To form a large speculum in one mass of such material seemed impossible, and Lord Rosse was led to the expedient of building one up of small segments, separately cast and polished, and united upon a foundation-plate formed of a brass specially prepared that its temperature expansion should be identical with that of the speculum metal. Repeated trials upon a small scale were made, and at length a mirror of 3-feet diameter was completed, which performed well upon small stars, but that exhibited its divisions upon the disk of a large star. In the course, however, of the foundry experiments upon plates from which to cut the parts of composite specula, Lord Rosse was led to a method of casting sound slabs of metal of large size at one operation. The points to be secured were, cooling without cracking, and freedom from pores or air-bubbles. The ordinary sand-mould allowed the edges of the

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plates to solidify first, and the central portions were confined and strained when their moment of cooling came, and this was the cause of cracking. It occurred to Lord Rosse that if the metal could be cooled from its lower surface upwards, in laminæ or strata, as it were, the upper surface being the last to solidify, this evil would be remedied. He tried an iron mould and threw a jet of water upon its under surface when the molten metal was within. This answered the immediate end, but the iron cracked and spoilt the casting. The next idea was to make the mould with a stout iron bottom and sand circumference, and trust to the abstraction of heat by the iron alone to cool the metal from below. This succeeded; but another evil betrayed itself: the air entangled between the speculum metal and the iron, formed large cavities that ruined the disk; in the porous sand-moulds it found a ready exit. The removal of this defect was the culminating triumph in this department of the noble founder's labours. Equal to every emergency he proceeded to make a ventilating bottom for his mould: he took thin bands of iron and closely packed them edgewise into a frame the size of the intended speculum, so that instead of a solid plate he had a surface somewhat like a book-edge; close enough to prevent the viscous metal from leaking out, but porous enough to allow the air to escape freely. The face of such a disk, turned in a lathe to the desired convexity and made smooth, formed the base of a mould, which, with a sand ring for its sides, permitted the casting of sixteen slabs for 3-feet specula, of which not one was defective. This improvement removed the last barrier to the casting of mirrors of any required diameter.

After meeting and surmounting more difficulties connected with the manufacture of crucibles, and after the exercise of anxious patience during the fortnight occupied by the cooling of the metal in an annealing oven, Lord Rosse obtained a 3-foot disk without fault or blemish, ready for the operations of grinding and polishing. It had been thought that the hand of a workman was the only fit guide for the polishing tool of a reflector, that the process was one which must be *felt* to be successfully followed. Such a notion was a manifest impediment to attempt, even in the manuafacture of large telescopes. Lord Rosse was compelled at an early stage of his experiments to resort to machine power. The manual operation of figuring a speculum is pretty well known: the tool is mounted upon a pedestal, and the workman, grasping the metal disk, gives it a rectilinear or curvilinear motion in a direction perpendicular to the plane of his body, at the same time walking round the pedestal. To reproduce these combined motions, with the means of methodically altering them and maintaining them in their altered condition, was the object of the polishing apparatus invented by Lord Rosse in 1828, and which was employed, with small modifications, in the many hundred grindings and polishings which he caused to be performed. The drawing and description of the machine are to be found in

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the *Philosophical Transactions* for 1840, and elsewhere; it is not necessary that we should describe it further than by saying that while the speculum rotates horizontally in a bath of water, for preservation of a uniform temperature, the polishing tool, weighted as may be necessary, lies upon it, and moves either in a straight line or in an ellipse the relation of whose axis is varied by adjustible eccentrics carrying the rod to which the tool is attached. With this mechanism every motion could be defined, and its effect upon the speculum's figure ascertained; a particular result once obtained could be repeated at will; and a precision in the figuring of reflectors was secured which, till this system was introduced, had been out of reach.

All difficulties of grinding overcome, and the figure tested by the image of a watch dial carried on a mast fifty feet above the polishing machine—so that the trials could be made without moving the speculum—new obstacles arose in the process of polishing. The grinding tool was covered with pitch to form the polisher, but the unequal expansion of the bituminous layer, by pressure and rubbing, destroyed its form, so that it would no longer fit the speculum, but chafed it untruly. This difficulty, never felt in working small mirrors, because the pitch layer can expand equally where its area is small, was a puzzling one to Lord Rosse working upon his large scale. But he overcame it at last, by scoring the resinous surface of the tool with grooves, and subsequently, as even this plan sometimes failed, by furrowing the iron tool itself. So long as the pitch was soft enough to escape into the grooves, all went well, but this softness was an impediment to the production of a true figure, which required that the medium for holding the particles of polishing powder should be as hard as possible. It takes a short time to relate what took a long time to invent. Two opposite properties, hardness and softness, were united in the resinous coating: first a layer of soft resin—which Lord Rosse eventually preferred to pitch—was applied, and then a layer of hard; the first acted as a cushion to the second, and thus the requisite conditions were realised. This was the final remedy of the final difficulty in the way of constructing great reflectors.

Such is an imperfect outline of the major obstacles that the Earl of Rosse encountered and overthrew during the twelve years ending with 1840, the date at which the 3-foot speculum was finally perfected. Of the minor embarrassments and perplexities we know nothing; but they must have been numberless. Throughout his original memoir are scattered modest sentences such as "after many difficulties" or "as the result of repeated trials," which must cover cares and anxieties beyond the appreciation of those who have not in some degree experienced similar troubles. The perfection of a great invention or a great instrument is rarely the work of a single labourer, but to the honour of Lord Rosse be it recorded that, unaided, he perfected the reflecting telescope. The 3-foot mirror, supported and mounted, surpassed, so far as

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comparisons could be inferred, the definition and power of the larger telescope of Sir William Herschel, and this was the only predecessor that could be considered to compete with it.

Yet after all it was but the stepping-stone to higher ends. Lord Rosse now saw no impossibility in doubling the dimensions he had already attained, and he immediately determined upon attempting, or rather achieving—for with him the words were synonymous—a 6-foot speculum. Undeterred by predictions of failure, he set about the task. The leap from a mirror weighing a ton and a quarter to one whose weight would be four tons and a half involved new contrivances and precautions. But so thoroughly had Lord Rosse mastered every point of the process that every difficulty could be foreseen and provided for. This was done so completely that no trying back was necessary, no single operation or piece of work had to be repeated. The principles of forming the mirror were perfectly similar to those worked out for the 3-foot; only enlargement of means were necessary. The crucibles and melting furnaces had to be multiplied, and an annealing oven specially built to receive the stupendous casting; and the grinding and polishing machine had to be adapted to the larger size. The mounting of the instrument necessarily differed considerably from that of the 3-foot, which was similar to the older instruments of Herschel and Ramage. It was evident that the pyramidal frame of ladders which gave stability in their case would fail upon the scale of their giant successor. So it was resolved to sacrifice a large part of the sky view, and mount the instrument between two walls or piers, thus limiting its range to half an hour on each side of the meridian, at the equator, but giving it range in altitude from near the south horizon to below the pole. It may be that in this restriction we have an indication of the approach to impracticable magnitude, although Lord Rosse affirmed that a larger reflector than this could be made and would be of service, and held that where observations are systematically carried on the confinement in Right Ascension was no great disadvantage.

The great speculum was cast on the 13th of April, 1842, the eve, by the way, of his lordship's wedding anniversary, and was completely mounted for use by February, 1845, when Dr. Robinson and Sir James South visited it to make some inaugural observations. The latter observer gave his impressions in a lengthy letter to the *Times* of April 16 of that year. His comments are full of enthusiasm and admiration for the sights which he beheld, and which he avowed had never been gazed upon by man before. Dr. Robinson's experiences were given verbally to the Royal Irish Academy almost upon the same day. We need not dwell upon his remarks, interesting and instructive as they are, for in the descriptions of the instrument's work during the subsequent years of its use we have a wider view of its capabilities. We have seen how it has with certainty resolved certain of the nebulæ, and how, for a while, till spectrum analysis arose to arrest the conclusions it

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tempted, it threatened to disprove the existence of such things as nebulæ at all by bringing all the objects so called within the domain of star-clusters; and we have seen how it has in this same class of bodies revealed a structure and arrangement more wonderful and inexplicable than anything which had hitherto been known to exist—we allude to the spiral conformation in all its varying conditions; we have seen, too, how it has necessitated some modifications in our previous ideas regarding planetary nebulæ, by breaking up the symmetric outlines that gave them their distinctive name. Lastly, we have seen, in the memoir recently published, that the views it has given of the nebula in *Orion* at intervals of fifteen years, as recorded by trustworthy draughtsmen, betray strong indications of change in the disposition of certain parts of the nebulous matter. On this point, as on many others, the instrument may yet yield results surpassing anything yet reaped from it; indeed its work, presided over by a new master inheriting the astronomical tastes of its founder, may be said to have but just begun. Already the spectroscope has been applied to it, and a few steps have been taken into that field of research for which, from its light-grasping power, it is so peculiarly fitted—the analysis of the light of the fainter stars and nebulæ. To pursue this a clock-movement is necessary; one is being, or by this time has been applied; so that ere long we may reap a harvest of knowledge upon a subject that was unknown when the instrument was designed, and thus will be added new honour to the fame of its designer.

The scientific life of the Earl of Rosse is the history of the leviathan telescopes. Their achievements are his renown. Posterity will esteem him rather as an engineering than as an observing astronomer. The honour that in the department of mechanical science we should accord to the great machine-tool makers who have rendered our age famous belongs in the department of astronomy to him. There is no disparagement implied in this comparison: far otherwise. It may justly be urged that the maker is above the user of an implement. Eyes are common to us all, all could make discoveries if they had the means. It was the means that the Earl of Rosse supplied. In his instruments we have the fruits of varied resources that are seldom combined in a single individual; great mechanical ability, optical and general scientific knowledge, and a large fortune; to these may be added indomitable perseverance and an imperturbable temper. Without these last the works he projected must have broken down. Be it remembered that he had no skilled workmen to help him, but drew all his personal assistance from the labourers about his estate, and he had to educate every man to the work. Everything was done in his own workshops and laboratory, even his steam-engine was of home construction, and at one time he was compelled to cast his own crucibles for the founding of his specula. But for the happy union of tastes, abilities, and means, such an instrument as the 6-foot reflector could not, in its time at least, have been

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brought into existence; the undertaking would have been beyond the powers of professional instrument-makers, and beyond the purse of any individual or body likely to be interested in its prosecution. Since, however, its maker proved the possibility of producing perfect mirrors of immense size by mechanical means, others, profiting by his experience and advice, fully published and freely imparted, have excelled in the work, and now a 4-foot reflector, constructed by Mr. Grubb, under the superintendence of a committee, of whom Lord Rosse was one, has been sent to Australia to re-explore the depths of the Southern skies. Lord Rosse's freedom in making his methods accessible to the world will be remembered with gratitude by those who may follow him in the same field of labour; and by those of us who have no such object it must not be overlooked, for it shows us that his aim was not the vainglorious possession of something beyond the reach of his fellow-men—in which case he would have preserved silence—but the extension of Astronomical research in his own and succeeding generations.

His Lordship joined this Society in 1824, and the Royal Society in 1831; he was President of the latter from 1849 to 1854, and received its Royal Medal, for his telescopic triumphs, in 1851. He presided over the British Association Meeting at Cork in 1843; and he was a Visitor of the Maynooth College and a Member of the Visiting Board of the Greenwich Observatory. During the last six years of his life he filled the post of Chancellor of Trinity College, Dublin. He died at his seat and the scene of his life's labours, Birr Castle, Parsonstown, King's County, on the 31st of October, 1867, from the effects of a surgical operation—the removal of a tumour produced by a slight sprain of the knee. His remains were deposited in the Church of St. Brandon, Parsonstown, having been followed to their resting-place by some 4000 of the tenantry on his estates, by whom he had been deservedly beloved. [J. C.]

Thomas Cooke, by the combined force of his character and his genius, raised himself from a very humble sphere in an obscure village to a foremost place among that long line of eminent opticians who have contributed in no small degree to sustain the high reputation of England for practical science.

He was born on the 8th of March, 1807, at Allerthorpe, in the East Riding of Yorkshire; his father being a shoemaker in that village. So straitened were the circumstances of his home, that, although the boy in many ways showed tokens of no common abilities, nevertheless his father could afford him no better education than that provided by the National School, and even this instruction was limited to the space of two years. For this reason he was, at an early age, put to his father's trade, but for this trade young Cooke appears from the first to have felt an unconquerable aversion. The boy was for ever thinking of

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Astronomy rather than of the mending of shoes. In those days, some of us can well remember how the voyages of Captain Cook exercised a wonderful fascination over young minds, and even now they might with great advantage displace much of the current literature of the times: over young Cooke's mind the fascination was irresistible. His bent was for the sea, and he would become, he said, a second Captain Cooke. But even here that common sense and practical side of the boy's character presented itself, which at later periods of his life became so salient a part of the character of the man. Thus, in selecting for himself the life of a sailor, he was so far from being a visionary, that he set himself at once to learn navigation, in order that he might make himself a good seaman. When, however, all was prepared for the final step, and young Cooke was on the point of starting on his new career, the entreaties and the tears of his mother prevailed on his feelings, naturally affectionate, and he set himself to consider in what way he could provide for his own maintenance nearer home. He renewed his studies with such small aids as he could procure, and in his seventeenth year he opened a village school. In this occupation he persevered until the age of twenty-two. This was the chief period when young Cooke was educating his own mind, and those who in after years had the pleasure of freely conversing with him on a variety of topics, must have been very sensible of the solid and successful nature of the result.

About the year 1829 he removed from Allerthorpe to York, where, for the next seven years he supported himself partly as an assistant-master in schools, and partly by private tuition. Such time as he could spare from this laborious and more remunerative occupation he devoted to mathematics and practical mechanics. In due course his thoughts were directed to the science of Optics, and, as was natural to one of his practical turn of mind, he attempted the construction of a reflecting telescope. Not satisfied with what he considered the uncertainty of the results of this form of instrument, and his pecuniary resources being unequal to the purchase of the requisite metals, he turned his attention to the manufacture of an achromatic object-glass. The material for one of the lenses he found in the bottom of an ordinary drinking glass; and although he had never seen the practical methods of grinding and polishing lenses, he at length succeeded, after many and wearisome trials, in producing a small achromatic telescope, the performance of which was sufficiently satisfactory to encourage him to further efforts. This telescope was subsequently purchased by Professor Phillips, of Oxford, who at that time was the centre of intellectual activity in York: no wonder that he was then, and continued to the last, the kind, warm friend of such a man as Cooke. For the counsel and encouragement afforded by that eminent *savant*, Mr. Cooke often expressed his gratitude in the hearing of the writer of this notice. Other friends also in York came forward, and held out the right hand of

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fellowship to one in whom they discerned the evidences of no ordinary gifts.

With much hesitation and misgiving, Mr. Cooke now exchanged his avocation of a private teacher, the pecuniary returns of which, however inconsiderable, were in a measure certain, for the greater uncertainties of the business of an optician in York. Mr. William Gray, a Fellow of this Society, had the honour of giving him his first considerable order. It was for an equatoreally mounted telescope of $4\frac{1}{2}$ inches aperture. Thirty years ago there were scarcely any telescopes of even that moderate aperture in this country, executed by English artists. It was not on account of any want of skill on the part of our native opticians, but mainly owing to unwise fiscal arrangements in relation to the excise on glass, that it was then customary to send for the few considerable telescopes required, to the successors of Frauenhofer at Munich. Hence it was nothing short of an important event at that time, for an English artist to undertake the construction of a large telescope, and still more so for a man, as yet unknown to fame, and living in a provincial town. Since that day matters in this respect have greatly improved, and at the present moment there is probably no astronomical or optical instrument which cannot be procured from one or another of our great native artists, on pecuniary terms quite as favourable, and of a quality at least as excellent, as any that can be procured from abroad. In effecting this result for the achromatic telescope, Mr. Cooke was a great pioneer.

Mr. Cooke appears to have considered that the success attending Mr. Gray's telescope laid the foundation of his fame as an optician. In 1851, an order was given by Mr. Pattinson of Gateshead, for an equatoreal instrument completely mounted in every respect, and of the then considerable aperture of 7 inches. This instrument, by its excellence, greatly contributed to the growing reputation of the artist. It was this telescope which was so munificently placed by its owner at the disposal of Professor Piazzi Smyth for the purposes of his well-known expedition to Teneriffe.

Orders now flowed to Mr. Cooke for specimens of his art, which more than tasked his limited means to supply. In consequence of this agreeable pressure, he purchased some land in Bishophill, York, and there erected those workshops which are now known as the Buckingham Works. Thither he carried his business in 1855, with but five or six workmen and one apprentice: at the present time they furnish employment for upwards of one hundred persons.

Mr. Cooke's reputation was thus established, and in the course of a very few years he constructed no less a number than nine complete equatoreals, having apertures varying from 8 to 10 inches. Some twelve or thirteen others were also constructed, varying from 5 to 8 inches in aperture, the smallest of which deserved the name of a noble instrument. It is a fact worthy

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of remark that all these large telescopes were commissions given to him by *private individuals*, living in a country of so small area as our own.

In the successful manufacture of the telescope with its elaborate mounting, Mr. Cooke possessed the great advantage of being not alone an accomplished and theoretical optician, but at the same time a mechanical engineer of no ordinary ability. Thus he seems to have been the first person to apply the ordinary means and machinery of mechanical engineering to the construction of optical instruments, treating them as accurate machines rather than as delicate instruments to be constructed solely by the hand. He made his own tools, and for the most part cast his own brass.

During the last five or six years of his life, Mr. Cooke turned his attention to the subject of circular graduation, and, after studying all the best existing machines for executing that most delicate kind of work, constructed for his own use a self-acting dividing engine of nearly 3-feet diameter. In this beautiful machine there are many novelties; the all-important process for ratching the edge of the main circle, the form of the vertical axis on which it revolves, the means of centring the circle to be operated on, and the gear for working the cutting tool, all differing from anything hitherto similarly employed, exhibit innumerable examples of Mr. Cooke's sound mechanical knowledge prompted by his rare fertility of invention. A larger machine, more perfect still, and capable of dividing circles of 4-feet in diameter, was designed and in part constructed by him, but its completion devolves on his successors. The self-acting engine formed part of the plant necessary to enable Mr. Cooke to carry out, under his own eye, the considerable orders for theodolites and other surveying instruments which latterly were given him by the Indian Government. Into this class of instruments he introduced many valuable improvements, and the workmanship which he bestowed on them, as on every thing that came from his hands, has rarely been equalled—never excelled.

Mr. Cooke was a horologist of no mean pretensions. Nearly one hundred turret-clocks, erected in churches and public institutions in all parts of England, testify by the accuracy of their performance to the exactness of their maker's mind. He brought to great perfection astronomical clocks, not only those of the ordinary kind, but also clocks exhibiting, under the regulation of a single pendulum, both sidereal and solar time on the same dial.

There was in Mr. Cooke's temperament no small admixture of the artistic element; a certain grace of form and harmony of proportion will be found to characterise all his works,—ennobling, by beautifying, the structure reared on rigid principles. Nor was he indifferent to the convenience of the too often forgotten observer. He was the first to employ, on the suggestion of his friend Lieut.-Col. Strange, machinery for engraving the

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figures on his instruments. By means of a small and simple engine the entire figures, of a standard form of maximum visibility, are engraved on a circle in less than two hours by an ordinary mechanic, with an elegance and precision unattainable by the most skilful unaided hand.

The first important meridional instruments made by Mr. Cooke were a pair of 5-feet transits destined to be employed in the determination of longitude by the officers of the Great Trigonometrical Survey of India. These are probably the largest portable instruments of their class in existence; and in some other respects they are unique. The transit axis are of aluminium bronze, an alloy selected for its immense rigidity; the appliances for effecting the principal adjustments are peculiar; and the mode in which no less than four levels are employed for effecting and watching the horizontality of the axis, and for determining its variations of flexure for different altitudes, contrived exclusively by Mr. Cooke, involves principles and presents features worthy of imitation. These fine instruments are briefly described by Lieut.-Col. A. Strange, under whose superintendence they were constructed, in the *Monthly Notices* for April and May, 1865, and in the *Proceedings of the Royal Society*, No. 90, 1867.

In September, 1863, Mr. Newall, of Gateshead, gave Mr. Cooke the princely commission to construct for him the largest refracting telescope that had ever been proposed with the least probability of a successful issue; it was a gigantic equatoreal of no less than 25 inches clear aperture! At the present time we believe the largest completely mounted equatoreal does not exceed 15 French inches in aperture; the stride from even these magnificent machines to the dimensions of that undertaken by Mr. Cooke is simply enormous. The accurate figuring and polishing of such an object-glass requires an amount of skill, patience, mechanical resource, and self-reliance, of which no person not conversant with the innumerable difficulties besetting the attempt can form a conception. It may be sufficient here to say that in the present instance it was necessary to float the huge lenses of the telescope on quicksilver, lest some undue or unequal pressure during the act of polishing their surfaces should disturb their forms. There were also other mechanical contrivances of great ingenuity, which upon the final completion of the instrument, Mr. Cooke proposed to communicate to the scientific public without reserve. The object-glass was finished early in 1868, and the mounting is now nearly complete. The telescope is driven by a clock, the regulation of which is partly effected by the introduction of a pendulum escapement, producing an uniformity of motion which probably has not heretofore been obtained by other means. The description of this clock will be found in the *Monthly Notices* for 1868. The object-glass is said to be a complete success; if that on further trial prove to be the case, a work will have been accomplished which, for its magnitude and difficulty, is unexampled in the history of Astro-

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nomical Instruments. A noble transit-circle of 7 inches aperture is to accompany this imperial philosophical machine.

When Troughton completed his magnificent transit instrument for the Royal Observatory at Greenwich, he inscribed his honoured name upon it with very pardonable pride; in his days it was unique in the world, for all days it will be a noble monument of his skill. But this beautiful work of the great optician would look like a toy by the side of Cooke's gigantic equatoreal. The name of its constructor will ere long be engraved on this mighty instrument; it may well serve as the epitaph for his tomb, for the eye of Thomas Cooke was not permitted to see the completion of the work which his own genius had devised. Worn out, partly by the days and nights of anxiety which were unavoidably incident on so vast an undertaking, and partly by the struggles which always accompany the efforts of a man who has to make his own fortunes unaided in the world, he sank to his rest on the 19th of October, 1868, in the 62nd year of his age.

For some years past the great York factory has been practically superintended by two of Mr. Cooke's sons — Mr. Thomas Cooke, the depository of his father's profound knowledge, an accomplished optician; and Mr. Frederick Cooke, a mechanician of the highest attainments. The traditions and the methods of the house are in safe hands,— in the hands of those who will spare no effort to maintain and augment the fame which has become their most valued inheritance.

Those who had the advantage of knowing Thomas Cooke could not fail to admire him for his attainments and his genius; for the genuine truthfulness and simplicity of his character he won their love. Unlike most self-educated men, he was singularly modest in the appreciation of his own powers; unlike many men who have had long to struggle for eminence in their profession, he was singularly free from the depreciation of the abilities of his rivals. The foundation of his future skill as an optician he had laid in early life by a diligent application to mathematical studies, and it was impossible for any competent person to converse with him on the subject of his favourite science without being struck with his profound knowledge of every branch of its theory, while it was evident that he had put that theory to the test, and in some important instances had improved it by his experiments. It was owing to this happy combination of theory with experiment that the late Mr. Dawes was enabled to pass his well-known and well-deserved eulogy on the peculiar character of the achromatism of his telescopes.

England has acquired a well-merited reputation for its long and unequalled succession of eminent constructors of astronomical instruments. To the great names of the past, such as Dollond, Bird, Ramsden, Troughton and Simms, we must now in gratitude add that of Thomas Cooke. Happily there are those still living among us who possess the promise and the power of emulating his fame. [C. P.]

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Proceedings of various Observatories.

Royal Observatory, Greenwich.

Since the date of the last Report the principal special work at the Royal Observatory has been the reduction of the great bulk of the preparatory work for the new Seven-year Catalogue of Stars, which will contain the results of all the stellar observations made with the Transit-Circle, from 1861, January 1, to 1867, December 31. The star-constants, precessions, and secular variations, have been computed afresh for the adopted epoch (1864), and considerable progress is made in the reduction of the yearly values to the same epoch.

The addition of a labour of this kind to the ordinary office-work of the Observatory is always sufficient to absorb a large proportion of the computing-staff of the establishment. The work is, however, performed without any serious interference with the usual routine computations. It is believed that the new Catalogue will be found to be exceedingly valuable to Astronomers, for it will contain not only the positions of all the standard stars, but also those of a considerable number of stars which had never been observed at Greenwich since the time of Dr. Bradley, and which are included in Bessel's *Fundamenta Astronomiæ*. Every star observed by Bradley, whose R.A. and N.P.D. are contained in that work, has now been re-observed at Greenwich since the Observatory has been under the direction of the present Astronomer Royal. We shall have, therefore, ample materials for the determination of the proper motions of the remaining Bradley's stars not previously included in the papers of the Rev. R. Main and Mr. Stone. The Catalogue will also include stars observed for special purposes, as, for example, at the request of Colonel Sir Henry James, Director of the Ordnance Survey, and of Colonel Walker, Superintendent of the Great Trigonometrical Survey of India.

The number of stars contained in the new Catalogue will be about 2760.

During the past year, the Sun, Moon, planets, and special list-stars, have been observed as usual with the Transit-Circle; and the Moon, with accompanying stars and collimator for the instrumental corrections, with the Altazimuth. The observations with the latter instrument have been very numerous during the past summer and autumn, the number of complete observations of the Moon having far exceeded that in the corresponding period of any previous year.

The printing of the volume of the *Greenwich Observations* for 1867 is in an advanced state, and the reductions of the observations made in 1868 are nearly completed.

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The transit of *Mercury* over the Sun's disk on the morning of November 5 was very successfully observed. The details of the observations are given in the *Monthly Notices*. Independently of the value of the observed times of egress, the phenomenon gave the observers an opportunity of noticing the formation of the black ligament, and of deciding the question of the reality of the existence of the *two* phenomena of real and apparent internal contacts, as explained by Mr. Stone, in his recent paper on the 'Rediscussion of the Observations of the Transit of *Venus* in 1769.'

For the express purpose of determining the heating powers of the stars, a delicate combination of a small thermo-electric pile and sensitive reflecting galvanometer has been arranged by Mr. Stone. This apparatus (Mr. Stone's private property) which was finished in 1868, December, has only been tried, on account of late unsettled weather, on two nights; and that merely sufficiently to test its applicability to the purpose for which it was arranged. There was no intention to have called public attention to the matter until quantitative results had been obtained. It now appears that Mr. Huggins had, in 1867, applied a similar arrangement to the same purpose. Under these circumstances it has been thought advisable to mention the perfect independence of Mr. Stone's work upon this subject.

Royal Observatory, Edinburgh.

The number of meteorological stations in Scotland, whose observations are computed here, remains at fifty-five, and these are still regularly prepared for and printed by the Registrar-General of Births, Deaths, &c. in Scotland, either every month, or quarter, or both.

The distribution of time to the public by time-ball, time-gun, and Jones's controlled clocks, continues as heretofore; also meridian observations of stars with Transit instruments and Mural Circle—the zero points for Right Ascension being adapted invariably from the Greenwich list of fundamental stars, both polar and equatorial, as most obligingly furnished by the Astronomer Royal, G. B. Airy, at the beginning of every year.

Radcliffe Observatory, Oxford.

No change has taken place in the organisation or subjects of observation at this Observatory during the past year. The Transit Circle has been actively employed, and the Heliometer whenever the weather would permit it, throughout the year 1868.

The state of the reductions and of the printing is kept up to pretty nearly the same state of forwardness as before, as will be shown by the following statement. All the Astronomical Observations of 1867 have been completely reduced and discussed, and

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the Catalogue of Stars, containing about 1400, observed in both elements. As usual, considerable pains have been bestowed upon the discussion of the observations of N.P.D., which are on the whole very satisfactory. The transits have been reduced to the end of the year 1868, but the observations of N.P.D. are less advanced than usual, on account of the labours bestowed on the Second Radcliffe Catalogue. This is now essentially complete, and the printing of it will commence very shortly. An elaborate comparison has been made of the positions of the stars in this Catalogue with the positions of stars in the Greenwich Catalogue of 2022 stars for 1860. The varying errors of the equinox at different hours of Right Ascension are very similar to those in the First Radcliffe Catalogue; and, with regard to the N.P.D.s, the differences are small, but there is a curious law of periodical errors (the stars being arranged in order of polar distance) very similar to that exhibited in the observations made with the Carrington Transit Circle. Thus we have observations made with two distinct circles, and treated by two different astronomers, giving almost precisely the same errors when compared with the results of the Greenwich Transit Circle; and the differences are not accounted for in any great degree by the difference of the refraction constants which are employed in the reductions.

The printing has gone on at the usual rate, that is, the results of the Astronomical Observations for 1866 have been printed for some time, and the printing of the volume for 1867 has been begun. The Meteorological Reductions for 1866 are nearly finished, and then the volume will be completed.

While speaking of the meteorology, it may be mentioned that the observations of the wind for some years have been recently discussed by Mr. Baxendell of Manchester, who has arrived at a very curious result with reference to the well-known fact of the different average velocities of the wind during the day and night hours. Observing that during the night hours, till seven o'clock in the morning, the velocity is sensibly constant, but increasing very rapidly after that hour, he finds that the force put in action to produce this additional velocity is almost precisely at right angles to the direction of the magnetic meridian; and he considers that this force, from whatever cause derived, is too large to be neglected in any speculations or forecasts of the weather.

Cambridge Observatory.

The Transit and Circle Observations during the year have been limited almost wholly to the usual observations of the Sun about the times of the equinoxes and solstices; to observations of *Jupiter*, *Uranus*, *Neptune*, and *Iris*, near the oppositions, and to the observations of fundamental stars for the determination of instrumental and clock errors. *Iris* was observed for the purpose of testing the accuracy of the Ephemeris, which Dr. Brunnow has computed by means of his new and still unpub-

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lished Tables of that planet. These observations were made with great difficulty on account of the deficiency in optical power of our present meridian instruments.

The errors due to the form of the pivots of the Transit instrument have been carefully re-determined. A comparison with former determinations shows that the changes arising from the wear of the pivots are very slight.

Two very satisfactory series of observations of Brorsen's and Winnecke's Comets were taken with the Northumberland Equatoreal. The relative position and distance of *Algol* and its companion were determined at the request of Mr. Aldis.

The Equatoreal has also been used occasionally in observing occultations.

The courses of a considerable number of the August Meteors were observed by means of the two meteoroscopes.

The anemometer has been out of order more than once during the past year in consequence of the formation of ice in the interior of the box containing the wheel-work, by which the wheels have been stopped and their teeth injured. Measures have now been taken to prevent the accumulation of moisture in the interior for the future. With the exception of these interruptions in the determination of the velocity of the wind, the meteorological observations have been punctually registered.

The reductions and the preparation for the press have gone on steadily. The Transit Observations of several years are ready for the press, and part of the copy is in the printer's hands. Considerable progress has also been made in the copy for the press of the Mural Circle observations.

Nearly all the Equatorial observations of occultations and of comets made up to the present time are reduced.

The Meridian circle which Mr. Simms is constructing for this Observatory is now in a very forward state, and it is hoped that it will prove to be one of the finest instruments of its class. Preparations will shortly be made for mounting this instrument, which is to occupy the place of the present Transit instrument.

Liverpool Observatory, Bidston, Birkenhead.

An important alteration has recently been sanctioned by the Mersey Docks and Harbour Board with regard to the conditions on which chronometers are in future to be received at the Liverpool Observatory. Hitherto they have been left for an indefinite time, for the purpose of being rated, and a large amount of labour has thereby been thrown upon the observatory. In future, chronometers are to be received for such a time only as is found to be necessary for efficiently testing them. Subject to the regulations, chronometers belonging to ships paying dues to the Mersey Dock Estate are tested gratuitously, as are also those belonging to any chronometer-maker, who on application can now send such a num-

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ber to be tested as the Director of the Observatory may from time to time decide upon.

As a means of communicating time to the port and towns of Liverpool and Birkenhead the time-gun is found to be very successful. The position of the gun is such that a view of the flash is commanded for several miles both up and down the river Mersey, and the sound is well heard for a long distance on each side of the river.

The work in the hands of Messrs. Troughton and Simms has been greatly delayed, and is not yet completed.

Glasgow Observatory.

The observations at the Glasgow Observatory during the past year have been essentially the same as in former years. The Transit Circle is employed in the observation of a selected list of stars below the fifth magnitude. Greenwich time continues to be transmitted to the city and port of Glasgow by Jones's method of control. An improved form of micrometer by Troughton and Simms has been recently obtained for the Ochtertyre Equatoreal, and observations of the minor planets are now made with the instrument on every favourable night. A few observations of Brorsen's Comet were obtained on the occasion of its last return to the perihelion. Professor Grant finally reports that the system of meteorological operations established by the Meteorological Committee of the Royal Society, acting in concert with the Board of Trade, is now regularly prosecuted at the Observatory.

Royal Observatory, Cape of Good Hope.

A very careful series of observations of *Mercury* on the Sun's disk were made here by Mr. Mann. A very accurate determination of the errors of the differences in the tabular places of the planet and the Sun is thus afforded.

The November Meteors were well observed. These results will shortly appear in our *Notices*.

The reductions for a Catalogue are going on unflinchingly.

Kew Observatory.

During the past year the Kew Heliograph has been at work without interruption, except from unfavourable weather.

There were on the whole 174 days of observation, on which 285 sun-pictures were taken.

The measurements alluded to in the last Report are continued with the utmost rapidity consistent with that extreme accuracy which is necessary in determining the positions of spots, and at the present moment the heliographic positions, as well as the areas of three years of sun-spots, have been determined, commencing

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with February, 1862. The measurements for two of these three years are given in a paper (alluded to in the last Report), which was presented to the Royal Society on March 31st, 1868, and is entitled "Researches on Solar Physics; Heliographical Positions and Areas of Sun-spots, observed with the Kew Heliograph during the years 1862 and 1863." By Messrs. De La Rue, Stewart, and Loewy. The paper is at the present moment going through the press as part of the *Transactions of the Royal Society.*

The same authors communicated to the Royal Society in June, 1868, an account of certain observations and measurements of a spot which was fortunately photographed at Kew while in the act of passing round the Sun's limb. This paper is entitled "An Account of some recent Observations on a Sun-spot made at the Kew Observatory," and is also being printed in the *Transactions of the Royal Society.*

Besides these two papers there has been printed a short appendix to the second series of preliminary researches in Solar Physics. This paper exhibits the distribution in heliographic latitudes of the sun-spots observed by Carrington, and shows, on an average, the latitudes of sun-spots when *Venus* crosses the solar system as less than when she is at her greatest distance therefrom. It also shows that there is excessive solar action at those periods when *Venus* and *Mercury* are together, thus tending, in connexion with previous researches, to the conclusion that *Venus*, *Jupiter*, and *Mercury*, are the planets most influential as regards the production of spots.

Finally, two communications have been made to the Royal Astronomical Society; the one giving a representation of the appearance which the Sun presented at Kew on the day of the total eclipse, Aug. 18, 1868, and the other being the usual yearly comparison of the number of groups of spots observed at Kew and at Dessau during the past year.

The plan of work for the present year is, while continuing the photographic observations, to push on with the measurements; and it is hoped that before next Report the positions and areas of spots for two more years may have been made, thus comprising five years in all, from the commencement in 1862. And also that another paper, embodying these positions and areas, may have been communicated to the Royal Society.

It is also hoped that the observations on distortion may be repeated and reduced. At the present moment it is impossible to commence the series of actinic observations alluded to in last Report.

Lord Rosse's Observatory.

During the last year the observations on the nebulæ and clusters of stars in Sir John Herschel's Catalogue have been continued in the same manner as heretofore.

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Lord Rosse's present assistant has on two or three occasions examined the spectrum of the Great Nebula in *Orion*, and confirms previous suspicion that there exist in it other bright lines in addition to the three well-known ones; more particularly one on the more refrangible side of F (a position where it could not be confounded with the maximum brightness of a continuous spectrum); all these lines are, however, of extreme faintness.

During this season the planet *Jupiter* has been examined, and a few sketches made.

A series of drawings of *Mars* has also lately been begun.

Some experiments on lunar radiation have been commenced, which have been so far successful as to show that the thermo-electric pile is capable of detecting heat near the time of full moon. In consequence, however, of the various disturbing causes having been only partially eliminated, as yet no reliable estimate of its amount has been made; the hope is entertained that this may be done.

Mr. Huggins' Observatory.

Time has not been found during the past year to continue some experiments which were conducted in the Observatory in the early part of 1867, for the purpose of detecting the heat received from the stars, and also, if possible, from the Moon. It may be well, however, to state that though, in consequence of the great difficulty of avoiding disturbing influences, no trustworthy quantitative estimate was obtained, yet, in the case of the stars *Sirius*, *Regulus*, *Pollux*, and *Arcturus*, the observations made in the early part of 1867 showed that the very sensitive thermopile, which had been specially constructed for these experiments, was sensibly affected. The heat of the Moon was observed with less certainty. These observations will be continued.

In the early part of 1868, observations which had been for some time in progress for the purpose of ascertaining the motion of stars towards or from the Earth by means of alterations in the refrangibility of their light, were successfully concluded for *Sirius*.* The line in the spectrum of *Sirius*, at the position of Fraunhofer's F, and due to hydrogen, was found to have been shifted by the star's motion towards the red, to an amount such as to show that *Sirius* was moving from our system with a velocity of about 29 miles per second. But this amount has to be corrected for the solar motion in space.

Similar observations on the Great Nebula in *Orion* showed that the coincidence of two of the lines of its spectrum respectively, with a line of hydrogen and a line of nitrogen, was perfectly maintained in an apparatus in which a difference of wave length of 0·0460 millionth of a millimetre would have been

* See *Philosophical Transactions*, 1868, p. 529.

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detected. This nebula, therefore, was not receding with a velocity so great as 10 miles per second, for this motion added to that of the Earth would have caused a want of coincidence that would have been observed. If the nebula were approaching us, it might have had a velocity as great as 20 or 25 miles per second, as part of it would have been annulled by the Earth's motion in the opposite direction.

The method of placing the induction spark before the object-glass brought into notice a fact of some interest. The loss of brightness showed itself in the case of the spectra of hydrogen and nitrogen, by the total extinction of all the lines, with the exception of the one line in each spectrum, which is found in the nebulæ. If we should have reason to believe that lines in the nebulæ have been quenched on their way to us, we must consider it as an evidence of a power of extinction of light existing in cosmical space, and not as an effect of distance, since the nebulæ are objects of sensible size.

During the year two Comets have been subjected to prismatic examination. Brorsen's Comet gave a spectrum of three bright bands.* Comet II. 1868, gave also three luminous bands, which, though in similar parts of the spectrum, differed considerably in position and in character from those of Brorsen's Comet. The spectrum of shaded bands of Comet II. was found, by direct comparison in the instrument with the spectrum of olefiant gas, to be identical in refrangibility and in general characters with the spectrum of carbon.†

On April 15, 1868, the spectrum of a solar spot was examined. It was found that three-fourths of the apparent light of the umbra came from that region of the Sun, the remaining fourth from our atmosphere. Most of the lines were increased in width, but this was not the case with the lines C and F, which are due to hydrogen. This fact is of interest in connexion with what we now know to be the nature of the solar prominences, and may have been due to these objects lying over the umbra of the spot.

With the help of the information of the positions of the bright lines of the solar prominences, furnished by the eclipse of August 1868, these bright lines were found, at the first instant of looking for them, with a small spectroscope, with which they had been sought for previously in vain.

The appearances observed at the Transit of *Mercury* have been described in the *Monthly Notices*. For the purpose of throwing light upon the origin of the bright spot seen upon *Mercury*, an artificial disk of about the same apparent size as *Mercury* was viewed upon the Sun, but no bright spot could be detected upon it.

* *Proceedings of the Royal Society*, vol. xvi. p. 386.
† *Phil. Trans.* 1868.

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Mr. Lockyer's Observatory.

Mr. Lockyer's observatory work has of late been exclusively devoted to the Sun, and his recent discoveries have been alluded to in another part of this Report.

Mr. Lockyer's detailed account of his work, communicated to the Royal Society and the Paris Academy of Sciences, includes among other matters,—

I. The determination of the exact position and number of the bright lines observed in the red flames, the phenomena which characterise them, and the substance of which they are principally composed.

II. The determination of the fact that the red flames are merely local heapings up of an envelope which is continuous round the Sun.

III. The approximate determination of the pressures existent in the prominences and at the bottom of the continuous envelope.

IV. A discussion of the bearing of these discoveries on the received theory of the physical constitution of the Sun.

Mr. Lockyer has suggested the term "Chromosphere" for the region or shell of the luminous prominences, which he has shown to completely surround the limb of the uneclipsed Sun.

At the time that the spectroscope revealed to Mr. Lockyer the fact that the prominence spectrum was never absent, and that, in fact, the prominence matter formed a continuous envelope round the Sun, he was not aware that such an envelope had been suggested by previous observers. He has since found, however, that Professor Grant * had based such a suggestion on an elaborate investigation of the eclipses up to and including that of 1842. That Mr. Swan and Herr Littrow afterwards came to the same conclusion from the eclipse of 1851; and that, later still, M. Le Verrier † and Father Secchi both published, after the eclipse of 1860, the idea that there was such a continuous envelope, which is indicated in the discussion by Mr. De La Rue of his admirable photographs.‡

The experimental proof of the truth of these surmises is, however, due to Mr. Lockyer. In his paper it is stated that, under proper instrumental and atmospheric conditions, the spectrum of the chromosphere is always visible in every part of the Sun's periphery; its height, and the dimensions and shapes of several prominences, observed at different times, are given in the paper. One prominence, 3′ high, was observed on the 20th of October, 1868. The possibility of variations in the thickness of this envelope is suggested, and the phenomena presented by the star in *Corona* are referred to.

* *History of Physical Astronomy*, chap. ix.
† *Phil. Trans.* 1862, pp. 405, 406; also Plate XV.
‡ *Cosmos*, vol. xvii. p. 174.

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Mr. Lockyer also points out that the line F invariably expands (that the band of light gets wider and wider) as the Sun is approached, and that the C line and the D line do not widen; and he enlarges upon the importance of this fact, taken in connexion with the researches of Plücker, Hittorf, and Frankland, on the spectrum of hydrogen. It is in connexion with this widening out that Mr. Lockyer, in conjunction with Professor Frankland, has been already able to determine the pressure at the surfaces of the chromosphere.

The F line, in fact, appears like an arrow-head resting on the Sun, and Plücker and Hittorf have ascribed such a widening out of the green hydrogen line as is here indicated as due either to temperature or pressure. This widening out has been found to be a real curve of pressure, extending from the highest prominences down to the surface of the photosphere; and the prominences have been determined to be of the most excessive tenuity, and the base even of the chromosphere to be of very much less than the pressure of the Earth's atmosphere.

Messrs. Frankland and Lockyer suggest in their preliminary notes on the chemical extension, so to speak, of the Observatory work, that the facts and experiments seem to show that Kirchhoff's hypothesis is no longer tenable; that, in all probability, the chromosphere is the only extensive envelope, outside the photosphere, and that the region of selective absorption is either situated in the photosphere itself or extremely close to it. Further, that the photosphere is neither solid nor liquid, but gaseous; the most brilliant portions of it giving out bright lines and forming apparently an ever-varying shell—the tops of smaller convection currents carrying matters out of the more intensely absorptive region indicated by Mr. Lockyer in a paper communicated to the Royal Society in 1866.

Notices of some points of Interest connected with the Progress of Astronomy during the past Year.

Solar Eclipse of 1868, *August* 18.

The results obtained by the different observers are of such interest and importance that the principal observations which would not otherwise appear in our Transactions are given in considerable detail in the observers' own words.

It is with great satisfaction that the Council call the attention of the Fellows of the Society to the complete success of their own expedition;—a success for which the Fellows are much indebted to the skill and energy of the Superintendent, Major Tennant.

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The Astronomical Society's Expedition.

It will be in the recollection of our Fellows that at the last Anniversary Meeting it was stated that preparations had been made at the recommendation of the Council of our Society for the observation of the Total Eclipse of the Sun in India. The Astronomer Royal took a warm interest in the proposed observations, and addressed the Secretary of State for India on the subject. It was ultimately arranged that the expense of the expedition should be borne jointly by the Government of India and the Imperial Government. The superintendence of the expedition was intrusted to Major Tennant. It is with great satisfaction that the Council is able to announce that Major Tennant has been most deservedly and eminently successful.

The Report of Major Tennant's observations is now in the hands of the Society, and it is intended that it shall appear in the forthcoming volume of the *Transactions*, fully illustrated with facsimiles of the photographs taken at Guntoor, which it is proposed to enlarge photographically, in order that the details of the prominences may be seen more clearly than is possible in the small copies which accompany the paper. Mr. De La Rue, who evinced considerable interest in the expedition and afforded facilities to Major Tennant for familiarising himself with astronomical photography before he started, has undertaken to see that the photographs are properly enlarged and copied.

It is here proper to state that to Major Tennant is due the credit of having first called attention to the peculiarly favourable conditions which would be presented by the Solar Eclipse of August 1868.*

It is only justice also to mention that, as far as regards the part which England took in the observations, it was mainly attributable to the energetic, active, and untiring zeal of Major Tennant, who happened to be in England on leave during the greater part of 1867, and who devoted much time in promoting the observations which, in spite of many difficulties, have been so successfully undertaken and carried out.

It will be recollected that Major Tennant, after consulting with the Astronomer Royal and other Fellows of the Society, undertook the following work. It was most comprehensive, and entailed possibly almost too much responsibility for the director of a single expedition.

1st. The determination of the geographical position of the station. This was successfully accomplished by means of a repeating-circle, although, in consequence of bad weather, there were not many available days between the arrival of the observers and instruments at Guntoor and the day of the eclipse. The position was found to be, Latitude N. $16^{\circ}\ 17'\ 29''{\cdot}23$, and Longitude E. $5^{h}\ 21^{m}\ 48^{s}{\cdot}6$.

* *Monthly Notices*, vol. xxvii. pp. 79, 174.

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Captain Branfill, R.E., subsequently connected the station with the marks of the Great Trigonometrical Survey, and deduced the following result: Lat. N. 16° 17′ 34″·3, and Long. E. $5^h\ 21^m\ 46^s{\cdot}5$.

2nd. Spectroscopic Observations. These were undertaken by Major Tennant himself, by means of the Sheepshanks' equatoreal, of 4·6 inch aperture and 5 feet focal length. This had been mounted equatoreally by the late Mr. Cooke, and was suitable for all latitudes in the British Isles, but it had to be altered to suit the more southern stations of India. The spectroscope employed with the telescope was made by Messrs. Troughton and Simms, and was provided with a scale of equal parts, which was illuminated by means of a lamp. The addition of this spectroscope threw additional work on the driving clock beyond that for which it was originally calculated, and, in consequence, some difficulties were experienced just at the critical time of observation from the irregularity of its going.

In spite, however, of this and other mishaps Major Tennant was able to carry out his observations, and ascertained, 1st, that the corona only gave the continuous solar spectrum. 2nd, that the light of the prominences was resolvable into certain bright lines of definite refrangibility, showing that these appendages consist of gaseous matter at a very high temperature. Major Tennant states that the Great Horn gave a beautiful line in the red, a line in the orange, and one in the green, which appeared multiple, also a line seen with difficulty near F; he says the red and yellow lines were evidently C and D, the reading of the bright line coincides with that of the brightest line in *b*. The line near to F was, in all probability, F itself; E, he says, was certainly not seen by him, and that, as regards the line in the blue, it was useless from his data to speculate upon it.

We now have more precise information from the researches of M. Janssen and Mr. Lockyer respecting the position of the bright lines, and the probable nature of the Sun's appendages; but it must be admitted that Major Tennant did this part of his work well, especially when the scope of the instruments at disposal is taken into account.

3rd. Major Tennant noted the time of first contact of the Sun and Moon's limbs by means of the repeating-circle at $6^h\ 2^m\ 12^s{\cdot}60$, sidereal time, and that of the last contact at $8^h\ 47^m\ 24^s{\cdot}62$, sidereal time.

4th. Polariscope. This part of the work was most ably performed by Captain Branfill, who joined Major Tennant early in August, and immediately set to work to familiarise himself with the phenomena produced by polarised light in the telescope. This instrument was one of the old collimators of the Great Transit Circle of Greenwich, and was lent by the Astronomer Royal. It was mounted on a polar axis, so that with one movement it could be made to follow the apparent motion of the Sun, but it was not provided with a driving-clock. To this telescope

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a polariscope eye-piece had been fitted by Mr. Ladd. The polarising apparatus comprised several combinations which could readily and rapidly be substituted one for the other. All these concurred in showing that the prominences (the Great Horn was chiefly observed) gave no indication of polarised light; on the other hand, every arrangement brought out the fact that the light of the corona was polarised in a plane passing through the Sun's centre. These observations were therefore fully and successfully carried out, and left nothing to desire.

5th. We now come to the photographic observations; these were under the immediate direction of Sergeant Phillips, who is not only a skilled photographer, but also had the advantage (as well as the Sappers who aided him) of working in Mr. Warren De La Rue's observatory at Cranford. The telescope employed is a Newtonian with a silvered-glass mirror, 9 inches in diameter. by With, and specially mounted by Mr. Browning. Preparations had been made for having a very large field, in order that the corona might be depicted as well as the prominences. Unfortunately the Sun was covered with cumulo-stratus clouds which diminished the actinic power of the light of the corona so much that it was not recorded. In other respects the photographs (six in number) were eminently successful.

Interval. s			h m s		
58·5	No. 1	Exposed at	7 17 2·9	S. T. for	1ˢ
61·4	„ 2	„	18 1·4	„	5ˢ
58·1	„ 3	„	19 2·8	„	10ˢ
63·2	„ 4	„	20 0·9	„	5ˢ
90·7	„ 5	„	21 4·1	„	1ˢ
	„ 6	„	7 22 34·8	„	1ˢ

Paper copies of these, about 2 inches in diameter, accompany the Report; and Mr. James, one of Major Tennant's assistants, made excellent drawings of the Great Horn and other prominences seen in the photograph, by means of the microscope. Since the arrival of the memoir, Serjeant Phillips has brought safely to England eight sets of transparent copies on glass, which have been distributed to individuals and learned bodies; amongst others, to the Royal Society and the French Academy of Sciences. On the occasion of a lecture given by Prof. Herschel, at the Royal Institution, on January 22, these were shown by means of the electric lamp, and projected on a screen, on a scale of about 5 feet for the Moon's diameter. The amount of detail visible under these circumstances was very remarkable. The spiral structure of the Great Horn, to which Major Tennant has called attention, was very evident. This spiral formation Major Tennant ascribes to the confliction of an ascending current, and one at right angles to it. Since then Mr. Warren De La Rue has

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procured some very beautiful copies, about 6½ inches diameter. He has also discussed, graphically, the small paper photographs, and communicated the results to the Society.* The diagram accompanying his paper shows fairly the form and relative position of these appendages with respect to the Sun. Mr. Warren De La Rue thought that he had detected a rotation of the Great Horn on its axis during the intervals between occurrence of totality at the various stations along the line of the eclipse. He has since been favoured by Prof. Foerster, Director of the Berlin Observatory, with a copy of the first Aden photograph, and informs the Council that there does not appear to be any very great change in the appearance of the Great Horn at Aden and at Guntoor. This comparison of results brings out forcibly the great value of photography for this class of observations, for the most careful and collected observer is liable to make an error in recording eye-observations.

In justice, however, to Col. Addison, whom Major Tennant had induced to make observations at Aden, it must be stated that a drawing of the prominences which he sent to Major Tennant led that gentleman to conclude that no change had occurred in them between the epoch of Aden and that of Guntoor.

Major Tennant reached Aden on the 25th of January; as this was nearly the first place where observations of the totality could be made, he enlisted the services of Captain Davis, the Peninsular and Oriental Company's agent, and of an old companion, Major Napier, R.A. Both these gentlemen promised their aid, and he learnt from them that Col. Addison and Major Weir, H. M. 2nd Royal Regiment, would be likely to be valuable coadjutors. Unfortunately, the contemplated observations with the polariscope, spectroscope, and intended drawings of the corona, were rendered impossible, in consequence of clouds. But the prominences were seen and recorded with great accuracy, as it has been before stated.

The Council have every reason to feel satisfied with the steps they took in conjunction with the Astronomer Royal, in furthering Major Tennant's views, and in thus securing a most valuable series of observations.

Lieut. J. Herschel's Account. Position, Jamkandi.

The totality commenced unseen. "A few seconds more and the spectrum of diffuse light vanished also, and told me the eclipse was total, but behind a cloud. I went to the finder, removed the dark glass, and waited, how long I cannot say, perhaps half a minute. Soon the cloud hurried over, following the Moon's direction, and therefore revealing, first, the upper limb, with its scintillating corona, and then the lower. Instantly I marked a

* *Monthly Notices*, vol. xxix. p. 73.

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prominence near the needle-point, an object so conspicuous that I felt there was no need to take any precautions to secure identification. It was a long finger-like projection from the (real) lower left-hand portion of the circumference. A rapid turn of the declination-screw covered it with the needle-point, and in another instant I was at the spectroscope. A single glance, and the problem was solved.

"*Its Spectrum.*—Three vivid lines, red, orange, blue; no others, and no trace of a continuous spectrum.

"When I say the problem was solved, I am, of course, using language suited only to the excitement of the moment! It was still very far from solved, and I lost no time in applying myself to measurement. And here I hesitate, for the measurement was not effected with anything like the ease and certainty which ought to have been exhibited. Much may be attributed to haste and unsteadiness of hand, still more to the natural difficulty of measuring intermittent glimpses; but I am bound to confess that these causes were supplemented by a failure less excusable. I have no idea how those five minutes passed so quickly! Clouds were evidently passing continually, for the lines were only visible at intervals—not for one half the time certainly—and not always bright, but still I ought to have measured them all. My failure was in insufficient illuminating power, but why, I cannot tell. I never experienced any difficulty of the kind with the nebulæ, which required that I should flash in light suddenly over and over again. I had found the hand-lamp the surest way, but it failed me here in great measure. The red line must have been less vivid than the orange, for after a short attempt to measure it, I passed on to secure the latter. In this I succeeded to my satisfaction, and accordingly tried for the blue line. Here I was not so successful. The glimpses of light were rarer and feebler, the line itself growing shorter, and what remained of it further from the cross. I did, however, place the cross wires in a position certainly very near the true one, and got a reading before the re-illumination of the field told me that the Sun had reappeared on the other limb. These readings were called out, as those of the solar lines had been, to my recorder, and it was only afterwards that I compared them.

"I need not dwell on the feelings of distress and disappointment which I experienced on realizing the fact that the long-anticipated opportunity was gone, and, as it seemed to me then, *wasted*. I seemed to have failed entirely. Almost mechanically I directed the telescope to the bright limb, to verify the readings of the solar lines, and in doing so my interest was again awakened by the near coincidence, as it seemed, of the line F with the position of the wires; but a little reflection convinced me that the distance of the former was greater than the error which I might have made in intersecting the blue line. I read F, and then D and C. The following were my readings up and down:—

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	C	D	*b*	F
Before	1·91	2·96	4·58	5·64
	1·90	2 94	4·56	5·61
	1·93	2·98	4·60	5·65
	1·92	2·97	4·58	5·62
Bright lines		[3·00]		[5·56]
After	1·93	3·00		5·65

"I consider that there can be no question that the orange line was identical with D, so far as the capacity of the instrument to establish any such identity is concerned. I also consider that the identity of the blue line with F is not established; on the contrary, I believe that the former is less refracted than F, but not much. With regard to the red line, I hesitate very much in assigning an approximate place; B and C represent the limits, it might have been near C; I doubt it being so far as B. I am not prepared to hazard any more definite opinion about it. Its colour was a bright red. This estimate of its place is absolutely free from any reference to the origin of the lines C and F."

The spectrum of the corona does not appear to have been specially examined.

Lieut. Campbell's Report.

"The instruments in question were as follows: a telescope of 3-inch aperture, mounted on a rough double axis, admitting of motions in azimuth and altitude by hand only unaided by any appliance for clamping and slow motion. The telescope was provided with three eye-pieces of magnifying powers 27, 41, and 98; and with it were furnished two analysers for polarized light, viz., a double-image prism and a Savart's polariscope.

"On the first opportunity after the commencement of the total phase of the eclipse I turned on the double-image prism with the eye-piece of 27 magnifying power, as recommended in the Instructions, which gave a field of about 45′ diameter. A most decided difference of colour was at once apparent between the two images of the corona; but I could not make certain of any such difference in the case of a remarkable horn-like protuberance, of a bright red colour, situated about 210 degrees from the vertex, reckoned (as I have done in all cases) with reference to the actual, not the inverted image, and with direct motion. I then removed the double-image prism and applied Savart's polariscope, which gave bands at right angles to a tangent to the limb, distinct, but not bright, and with little, if any, appearance of colour. On turning the polariscope in its cell, the bands, instead of appearing to revolve on their own centre, passing through various phases of brightness, arrangement, &c. travelled bodily along the limb, always at right angles thereto, and without much change in intensity, or any at all in arrange-

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ment. The point at which they seemed strongest was about 140° from the vertex, and I recorded them as black centred. Believing that with a higher power and a smaller field I should find it easier to fix my attention on one point of the corona, and observe the phases of the bands at that point, I changed eye-pieces, applying that of 41 power. With this eye-piece the first clear instant showed the bands much brighter than before, coloured, and as tangents to the limb at a point about 200° from the vertex: but before I could determine anything further a cloud shut out the view, and a few seconds later a sudden rush of light told that the totality was over, though it was difficult to believe that five minutes had flown by since its commencement. I experienced a strong feeling of disappointment and want of success: the only points on which I can speak with any confidence being as follows:—

"(1.) When using the double-image prism, the strong difference of colour of the two images of the corona, and the absence of such difference in the case of the most prominent red flame. (2.) With Savart's polariscope the bands from the corona were decided: with a low power they were wanting in intensity and colour: excepting alternate black and white, making it difficult to specify the nature of the centre: and their position was at right angles to the limb, extending over about 30° of the circumference. When the polariscope was turned the bands travelled bodily round the limb without other changes in position or arrangement, as if, indeed, they were revolving round the centre of the Sun as an axis. With a higher power, when a smaller portion of the corona was embraced, the bands were brighter coloured, and seen in a different position, viz., tangents to the limb.

"The appearance observed with a low power seems exactly what might be expected supposing the bands to be brightest at every point when at right angles to the limb, in which case the bands growing into brightness at each succeeding point of the limb, would distract attention from those fading away at the points passed over as the analyzer revolved."

Rapport de M. Rayet.

"L'appareil dont je me suis servi à Wah-Tonne, pour l'examen optique de la lumière des protubérances, se composait d'un télescope à miroir de verre argenté, de 20 centimètres de diamètre, monté équatorialement pour la latitude de la station, et d'un spectroscope à vision directe. Ce dernier instrument, formé de trois prismes très-dispersifs, était combiné pour avoir une faible longueur et donner beaucoup de lumière.

"Spectre des cornes.—La fente du spectroscope ayant été orientée de manière à couper à angle droit l'image du croissant lumineux très-étroit qui devait rester quelques secondes avant l'obscurité totale, j'ai d'abord étudié la lumière de l'extrémité des

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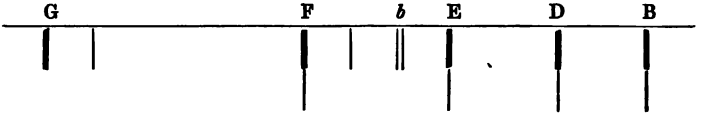
cornes. Sur le fond d'un spectre à raies obscures très-nettes, formé par la lumière atmosphérique diffuse, on voyait une bande beaucoup plus lumineuse, qui était le spectre de la lumière émise par l'extrémité de la corne. Quelque fût le peu de hauteur de cette partie on n'y distinguait rien de particulier. Les raies avaient un aspect (largeur et degré d'intensité) identique à celui des raies du spectre solaire ordinaire.

" L'observation des cornes a toutefois été interrompue quelques secondes avant l'éclipse totale, afin d'enlever les diaphragmes mis au téléscope, d'ouvrir légèrement la fente du spectroscope et d'être ainsi préparé à l'examen des protubérances.

" Spectre des Protubérances.—Dès l'instant de l'obscurité totale, la fente du spectroscope ayant été portée sur l'image de la longue protubérance qui venait de se montrer sur le bord oriental du Soleil, je vis immédiatement une série de neuf lignes brillantes, qui, d'après leur disposition dans le champ, leur espacement relatif, leur couleur et enfin par la physionomie même de leur ensemble, me semblent devoir être assimilées aux lignes principales du spectre solaire, B, D, E, *b*, une ligne inconnue, F, et deux lignes du groupe G. Ces lignes présentaient un très-grand éclat, et se détachaient vivement sur un fond gris cendré très-pâle.



" Les protubérances sont donc des jets d'une matière gazeuse incandescente, les flammes d'un phénomène chimique d'une puissance extrême. Il faut aussi remarquer que la lumière de la couronne est très-faible par rapport à celle des protubérances; car, tandis que la lumière de ces dernières donnait un spectre très-vif, la première, malgré l'ouverture assez grande de la fente, ne donnait aucun spectre coloré sensible.

" Pendant les observations précédentes, la fente du spectroscope était parallèle à la grande longueur de la protubérance; aussi voyait-on dans l'appareil des lignes lumineuses d'une hauteur sensible, en relation directe avec la hauteur de la protubérance. La fente ayant été tournée de 90 degrés, les raies se sont trouvées réduites à l'apparence de points brillants répondant à la faible largeur de la corne lumineuse. Il n'y a donc pas d'erreur d'observation possible, les lignes brillantes représentent bien le spectre de la lumière des protubérances.

" Le spectroscope étant dans la première position (fente parallèle à la longueur de la protubérance), les lignes très-vives, assimilées à D, E, et F, se prolongeaient au delà de la longueur moyenne par un trait lumineux très-faible; le spectre présentait l'apparence reproduite dans le dessin ci-joint. Une certaine portion de la matière gazeuse incandescente qui forme les pro-

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tubérances se répand donc dans l'atmosphère solaire au delà des limites que l'œil assigne en général à ces expansions.

" L'examen de cette première protubérance étant terminé, j'ai mis la fente sur la grande région lumineuse qui était à l'occident du Soleil. Le spectre s'est, cette fois encore, montré formé des lignes brillantes, disposées comme dans le premier cas, seulement je n'ai vu qu'une seule ligne violette. Toutes les protubérances ne semblent donc point émettre une lumière identique.

" M. Hall, ingénieur-hydrographe de la Marine de Saïgon, qui observait en un autre point de notre station, a également constaté que le spectre des protubérances était formé des lignes brillantes."

Rapport de M. Janssen.

" *Première partie.*—Le paquebot des Messageries impériales qui m'amenait de France m'a débarqué à Madras, le 16 Juillet, sur la côte de Coromandel.

"A Madras, j'ai été reçu par les autorités anglaises avec une grande courtoisie. Lord Napier, gouverneur de la province de Madras, me fit conduire à Masulipatam, sur un vapeur de l'Etat. M. Graham, collecteur adjoint, fut attaché à ma mission pour aplanir toutes les difficultés que je pourrais rencontrer dans l'intérieur.

" Il me restait à choisir ma station.

" Si on jette les yeux sur une carte de l'éclipse, on voit que la ligne de la centralité, après avoir traversé le golfe du Bengale, pénètre sur la côte Est du continent Indien à la hauteur de Masulipatam ; elle coupe les bouches de la Kistna, traverse de grandes plaines formées par le delta de ce fleuve et s'engage ensuite dans un pays élevé, contenant plusieurs chaînes situées à la frontière de l'Etat indépendant du Nizam.

"D'après l'ensemble des informations très-nombreuses recueillies et discutées, je fus conduit à choisir la ville de Guntoor, placée sur la ligne centrale à égale distance des montagnes et de la mer; j'évitais ainsi les brumes marines très-fréquentes à Masulipatam, et les nuages qui couronnent souvent les pics élevés.

"Guntoor est une ville indienne assez importante, centre d'un grand commerce de cotons. Ces cotons viennent en majeure partie des Etats du Nizam, et passent en Europe par les ports de Cocanada et Masulipatam. Plusieurs familles de négociants français résident à Guntoor ; elles descendent, pour la plupart, de ces anciennes et nombreuses familles qui, au siècle passé, faisaient fleurir nos belles colonies de l'Inde.

"Mon observatoire fut établi chez M. Jules Lefaucheur, qui voulut bien mettre à ma disposition tout le premier étage de sa maison, la plus élevée et la mieux située de Guntoor. Les pièces de ce premier étage communiquaient avec une large terrasse sur laquelle je fis élever une construction provisoire répondant aux exigences de nos observations.

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"Mes instruments consistaient en plusieurs grandes lunettes de 6 pouces d'ouverture et un télescope Foucault de 21 centimètres de diamètre.*

"Les lunettes étaient montées sur un même plateau qui les rendait solidaires. Le mouvement général était communiqué par un mécanisme construit par MM. Brunner frères, qui permettait de suivre le Soleil par un simple mouvement de rappel. L'appareil était muni de chercheurs de 2 pouces et $2\frac{3}{4}$ pouces d'ouverture formant eux-mêmes de bonnes lunettes astronomiques.

"En analyse spectrale céleste, les chercheurs ont une importance toute particulière; c'est par leur intermédiaire qu'on sait sur quel point précis de l'objet étudié se trouve la fente du spectroscope de la lunette principale. Il importe donc que les fils reticulaires, ou en général les points de repère placés dans le champ du chercheur, soient réglés très-rigoureusement sur la fente de l'appareil spectral. Tous mes soins avaient été apportés pour atteindre ce but capital. Des micromètres spéciaux devaient permettre, en outre, de mesurer rapidement la hauteur et l'angle de position des protubérances. Quant aux spectroscopes adaptés aux grandes lunettes, je les avais choisis de pouvoirs optiques différents, afin de pouvoir répondre aux diverses exigences des phénomènes de l'éclipse. Enfin, tout l'appareil † portait, du côté des oculaires, des écrans en toile noire formant chambre obscure, et destinés à conserver à la vue toute sa sensibilité.

"Indépendamment de ces instruments, consacrés à l'observation principale, j'avais apporté une riche collection de thermomètres d'une grande sensibilité, construits avec talent par M. Baudin,‡ des lunettes portatives, des hygromètres, baromètres, etc. Aussi ai-je pu utiliser le bon vouloir de MM. Jules, Arthur et Guillaume Lefaucheur, que se mirent à ma disposition pour les observations secondaires. M. Jules Lefaucheur, exercé au maniement du crayon, se chargea du dessin de l'éclipse. Une excellente lunette de 3 pouces, munie de réticules, fut mise à sa disposition; il s'en servit d'avance et s'exerça, sur des représentations artificielles d'éclipses, à reproduire d'une manière rapide et sûre les phénomènes qu'il aurait à représenter. La mesure des températures fut confiée à M. Arthur Lefaucheur, qui devait aussi au moment de la totalité, par une expérience très-simple de photométrie, nous faire connaître le pouvoir lumineux des protubérances et de l'auréole.

"J'étais assisté dans mes observations propres par M. Rédier, jeune aspirant au grade d'officier, que M. le commandant du

* "Le miroir de ce télescope avait été parabolisé par M. Martin, qui a voulu donner un concours désintéresse à notre expédition."

† "MM. Bardou et Secretan m'avaient obligeamment prêté deux des quatre objectifs de 6 pouces que j'avais avec moi. M. Bardou m'avait fourni la majeure partie des instruments de cet appareil. Je citerai aussi M. Wentzel pour le talent qu'il montre chaque jour dans le travail de mes prismes."

‡ "Parmi ces thermomètres s'en trouvait un construit sur mes indications par M. Baudin, sur le plan des thermomètres différentiels de M. Walferdin, mais dont le réservoir n'avait pas plus de 1 millimètre de diamètre."

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paquebot *l'Impératrice* avait bien voulu mettre à ma disposition. Le concours de M. Rédier, doué d'ailleurs dispositions heureuses pour les sciences d'observation, m'a été fort utile.

"Le temps qui nous resta avant l'éclipse fut employé à des études et des répétitions préliminaires; elles eurent l'avantage de familiariser tout le monde avec le maniement des instruments et me fournirent l'occasion de nombreux perfectionnements de détail.

"L'éclipse approchait et le temps ne semblait pas devoir nous favoriser. Il pleuvait depuis longtemps sur toute la côte. On considérait ces pluies comme exceptionnelles. Bien heureusement le temps se remit peu à peu avant le 18. Le jour de l'éclipse, le Soleil brilla dès son lever, bien qu'il fût encore dans une couche de vapeurs; il s'en dégagea bientôt, et au moment où nos lunettes nous signalaient le commencement de l'éclipse, il brillait de tout son éclat.

"Chacun était à son poste. Les observations commencèrent immédiatement.

"Pendant les premières phases, quelques légères vapeurs vinrent passer sur le Soleil; elles nuisirent à la netteté des mesures thermométriques; mais quand le moment de la totalité approcha, le ciel reprit une pureté suffisante.

"Cependant la lumière baissait visiblement; les objets semblaient éclairés par un clair de Lune. L'instant décisif approchait, et on l'attendait avec une certaine anxiété: cette anxiété n'ôtait rien à nos facultés, elle les surexcitait plutôt; et d'ailleurs elle se trouvait bien justifiée, et par la grandeur du phénomène que la nature nous préparait, et par le sentiment que les fruits de longs préparatifs et d'un grand voyage allaient dépendre d'une observation de quelques instants.

"Bientôt le disque solaire se trouve réduit à une mince faucille lumineuse. On redouble d'attention. Les fentes spectrales de l'appareil des 6 pouces sont rigoureusement tenues en contact avec la portion du limbe lunaire, que va éteindre les derniers rayons solaires de manière que ces fentes soient amenées par la Lune elle-même dans les plus basses régions de l'atmosphère solaire quand les deux disques seront tangents.

"L'obscurité a lieu tout à coup, et les phénomènes spectraux changent aussitôt d'une manière bien remarquable.

"Deux spectres formés de cinq ou six lignes très-brillants, rouge, jaune, verte, bleue, violette, occupent le champ spectral et remplacent l'image prismatique solaire qui vient de disparaître. Ces spectres, hauts d'environ 1 minute, se correspondent raie pour raie; ils sont séparés par un espace obscur où je ne distingue aucune raie brillante sensible.

"Le chercheur montre que ces deux spectres sont dus à deux magnifiques protubérances qui brillent maintenant à droite et à gauche de la ligne des contacts où vient d'avoir lieu l'extinction. L'une d'elles surtout, celle de gauche, est d'un hauteur de plus de 3 minutes; elle rappelle la flamme d'un feu de forge sortant

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avec force des ouvertures du combustible, poussée par la violence du vent. La protubérance de droite (bord occidental) présente l'apparence d'un massif de montagnes neigeuses dont la base reposerait sur le limbe de la Lune et qui seraient éclairées par un Soleil couchant. Ces apparences ont été décrites avec soin par M. Jules Lefaucheur; je ferai seulement remarquer, avant de quitter le sujet des protubérances sur lequel j'aurai à revenir d'une manière spéciale, que l'observation précédente montre immédiatement:

"1°. La nature gazeuse des protubérances (raies spectrales brillantes);

"2°. La similitude générale de leur composition chimique (spectres se correspondant raie pour raie);

"3°. Leur espèce chimique (les raies rouge et bleue de leur spectre n'étaient autres que les raies C et F du spectre solaire caractérisant, comme on sait, le gaz hydrogène).

"Je reviens maintenant à l'espace obscur qui séparait les deux spectres protubérantiels. On se rappelle qu'au moment de l'obscurité totale les fentes spectrales étaient tangentes aux deux disques solaire et lunaire; elles traversaient donc les régions circumsolaires immédiatement en contact avec la photosphère, régions où la théorie de M. Kirchhoff place l'atmosphère de vapeurs qui produisent par absorption élective les raies obscures du spectre solaire. Cette atmosphère de vapeurs, quand elle brille de sa lumière propre, doit, suivant la même théorie, donner le spectre solaire renversé, c'est-à-dire uniquement formé de raies brillantes. C'est le phénomène que nous attendions, ou du moins que nous cherchions à vérifier, et c'est pour rendre cette vérification décisive que j'avais accumulé tant de précautions. Mais on vient de voir que les protubérances seules donnèrent des spectres positifs ou à raies brillantes. Or, il est bien constaté que si une atmosphère formée de vapeurs de tous les corps qu'on a reconnus dans le Soleil existait réellement autour de la photosphère, elle eût donné un spectre au moins aussi brillant que celui des protubérances, formées de gaz beaucoup plus subtils et dès lors moins lumineux. Il faut donc admettre, ou que cette atmosphère n'existe pas, ou que sa hauteur est si faible qu'elle a échappé aux observations.

"Je dois dire, au reste, que ce résultat m'a peu surpris. Mes études sur le spectre solaire m'avaient amené à douter de la réalité d'une importante atmosphère autour du Soleil, et je suis de plus en plus porté à admettre que les phénomènes d'absorption élective rejetés par le grand physicien d'Heidelberg dans une atmosphère extérieure au Soleil ont lieu au sein même de la photosphère, dans les vapeurs où nagent les particules solides et liquides des nuages photosphériques. Cette manière de voir serait non-seulement en harmonie avec la belle théorie que nous devons à M. Faye sur la constitution de la photosphère, mais il semble même qu'elle en découle d'une manière nécessaire.

"En résumé, l'éclipse du 18 août a montré, suivant moi, que la constitution du spectre solaire est insuffisamment expliquée par

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la théorie admise jusqu'ici, et c'est dans le sens indiqué ci-dessus que je propose de le réviser.

" *Deuxième partie.* —Je reviens maintenant aux protubérances. Pendant l'obscurité totale, je fus frappé du vif éclat des raies protubérantielles : la pensée me vint aussitôt qu'il serait possible de les voir en dehors des éclipses; malheureusement le temps qui se couvrit après le dernier contact ne me permet de rien tenter pour ce jour-là. Pendant la nuit, la méthode et ses moyens d'exécution se formulèrent nettement dans mon esprit. Le lendemain 19, levé à 3 heures du matin, je fis tout disposer pour les nouvelles observations.

" Le Soleil se leva très-beau; aussitôt qu'il fut dégagé des plus basses vapeurs de l'horizon, je commençai à l'explorer. Voici comment je procédai. Par le moyen du chercheur de ma grande lunette, je plaçai la fente du spectroscope sur le bord du disque solaire dans les régions mêmes où la veille j'avais observé les protubérances lumineuses. Cette fente placée en partie sur le disque solaire et en partie en dehors donnait, par conséquent, deux spectres, celui du Soleil et celui de la région protubérantielle. L'éclat du spectre solaire était une grande difficulté; je la tournai en masquant dans le spectre solaire le jaune, le vert et le bleu, les portions les plus brillantes. Toute mon attention était dirigée sur la ligne C, obscure pour le Soleil, brillante pour la protubérance, et qui, repondant à une partie moins lumineuse du spectre, devait être beaucoup plus facilement perceptible.

" J'étais depuis peu de temps à étudier la région protubérantielle du bord occidental, quand j'aperçus tout à coup une petite raie rouge, brillante, de 1 à 2 minutes de hauteur, formant le prolongement rigoureux de la raie obscure C du spectre solaire. En faisant mouvoir la fente du spectroscope de manière à balayer méthodiquement la région que j'explorais, cette ligne persistait, mais elle se modifiait dans sa longueur et dans l'éclat de ses diverses parties, accusant ainsi une grande variabilité dans la hauteur et dans le pouvoir lumineux des diverses régions de la protubérance.

" Cette exploration fut reprise à trois reprises différentes et toujours la ligne brillante apparut dans les mêmes circonstances. M. Rédier, qui m'assistait avec beaucoup de zèle dans cette recherche, la vit comme moi, et bientôt nous pûmes même en prédire l'apparition par la seule connaissance des régions explorées. Peu après je constatai que la raie brillante F se montrait en même temps que C.

" Dans l'après-midi, je revins encore à la région étudiée le matin; les lignes brillantes s'y montrèrent de nouveau, mais elles accusaient de grands changements dans la distribution de la matière protubérancielle; les lignes se fractionnaient quelquefois en tronçons isolés qui ne se réunissaient pas à la ligne principale, malgré des déplacements de la fente d'exploration. Ce fait indiquait l'existence de nuages isolés qui s'étaient formés depuis le matin. Dans la région de la grande protubérance je trouvai

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quelques lignes brillantes, mais leur longueur et leur distribution accusaient, là aussi, de grands changements.

"Ainsi se trouvait démontrée la possibilité d'observer les raies des protubérances en dehors des éclipses et d'y trouver une méthode pour l'étude de ces corps.

"Ces premières observations montraient déjà que les coïncidences des raies C et F étaient bien réelles, et dès lors que l'hydrogène formait en effet la base de ces matières circumsolaires. Elles établissaient en outre la rapidité des changements que ces corps éprouvent, changements qui ne pouvaient être que pressentis pendant les si courtes observations des éclipses.

"Les jours suivants, je mis à profit toutes les occasions que pouvait m'offrir l'état du ciel pour appliquer la nouvelle méthode et la perfectionner, autant du moins que le permettaient les instruments, qui n'avaient pas été construits à ce point de vue tout nouveau.

"En suivant avec beaucoup d'attention les lignes protubérantielles, j'ai quelquefois observé qu'elles pénètrent dans les lignes obscures du spectre solaire, accusant ainsi un prolongement de la protubérance sur le globe solaire lui-même. Ce résultat était facile à prévoir, mais l'interposition de la Lune en eût toujours rendu la constatation impossible pendant les éclipses.

"Je rapporterai encore ici une observation faite le 4 septembre par un temps favorable, et qui montra avec quelle rapidité les protubérances se déforment et se déplacent.

"A 9^h 50^m l'exploration du Soleil indiquait un amas de matière protubérantielle dans la partie inférieure du disque. Pour en déterminer la figure, je me servis d'une méthode qu'on pourrait appeler chronométrique, parce que le temps y intervient comme élément de mesure.

"Dans cette méthode on place la lunette dans une position fixe, choisie de manière que, par l'effet du mouvement diurne, toutes les parties de la région à explorer viennent successivement se placer devant la fente du spectroscope. On note alors pour chaque instant déterminé la longueur et la situation des lignes protubérantielles qui se produisent successivement. Le temps que le disque solaire met à traverser la fente donne la valeur de la seconde en minute d'arc. Cette donnée, combinée avec la longueur des lignes protubérantielles estimées suivant la même unité,* fournit les éléments d'une représentation graphique de la protubérance.

"L'application de cette méthode à l'étude de la région solaire dont je viens de parler indiquait une protubérance s'étendant sur une longueur d'environ 30 degrés, dont 10 degrés à l'orient du diamètre vertical, et 20 degrés à l'occident. Vers l'extrémité de la portion occidentale, un nuage considérable s'élevait à $1\frac{1}{2}$ minute du globe solaire. Ce nuage long de plus de 2 minutes, large de 1 minute, s'étendait parallèlement au limbe. Une heure après (10^h

* "Cette estimation s'obtient d'une manière facile en plaçant sur la fente du spectroscope deux fils dont l'écartement, réglé sur le foyer de la lunette collectrice, représente un nombre déterminé de minutes d'arc."

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50m), un nouveau tracé montra que le nuage s'était élevé rapidement, prenant la forme globulaire. Mais les mouvements devinrent bientôt plus rapides encore, car dix minutes après, c'est-à-dire a 11 heures, le globe s'était énormément allongé dans le sens normal au limbe solaire ou perpendiculaire à la première direction. Un petit amas de matière s'en était détaché à la partie inférieure et se trouvait suspendu entre le Soleil et le nuage principal. Le temps, qui se couvrit, ne me permet pas de poursuivre davantage.

" Resumons ces observations.

" Considérée d'abord dans son principe, la nouvelle méthode repose sur la différence des propriétés spectrales de la lumière des protubérances et de la photosphère. La lumière photosphérique, émanée de particules solides ou liquides incandescentes, est incomparablement plus puissante que celle des protubérances due à un rayonnement gazeux. Aussi a-t-il été jusqu'ici à-peu-près impossible d'apercevoir les protubérances en dehors des éclipses. Mais on peut renverser les termes de la question en s'adressant à l'analyse spectrale. En effet, la lumière solaire se distribue par l'analyse dans toute l'étendue du spectre et, par là, s'affaiblit beaucoup. Les protubérances, au contraire, ne fournissent qu'un petit nombre de faisceaux dont l'intensité reste très-comparable aux rayons solaires correspondants. C'est ainsi que les raies protubérantielles sont perçues très-facilement dans un champ spectral, sous le spectre solaire, tandis que les images directes des protubérances sont comme écrasées par la lumière éblouissante de la photosphère.

" Une circonstance fort heureuse pour la nouvelle méthode vient s'ajouter à ces données favorables. En effet, les raies lumineuses des protubérances correspondent à des raies obscures du spectre solaire. Il en résulte que non-seulement on les aperçoit plus facilement dans le champ spectral sur les bords du spectre solaire, mais qu'il est même possible de les voir dans l'intérieur de ce spectre et, par conséquent, de suivre la trace des protubérances sur le globe solaire même.

" Au point de vue de la détermination de l'espèce chimique, les procédés suivis pendant les éclipses totales comportaient toujours quelque incertitude: en l'absence de la lumière solaire, on était obligé de recourir à l'intermédiaire des échelles pour fixer la position des raies des protubérances. La nouvelle méthode permet de comparer directement les raies protubérantielles aux raies solaires. Les identifications sont alors absolument certaines.

" Au point de vue des résultats obtenus pendant la courte période où elle a été appliquée, la méthode spectro-protubérantielle a permis de constater:—

" 1°. Que les protubérances lumineuses observées pendant les éclipses totales appartiennent incontestablement aux régions circumsolaires;

" 2°. Que ces corps sont formés d'hydrogène incandescent, et que ce gaz y prédomine s'il n'en forme la composition exclusive;

" 3°. Que ces corps circumsolaires sont le siége de mouvements dont aucun phénomène terrestre ne peut donner une idée; des

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amas de matière dont le volume est plusieurs centaines de fois plus grand que celui de la Terre, se déplaçant et changeant complètement de forme dans l'espace de quelques minutes.

"Tels sont les principaux résultats obtenus. J'espère que malgré l'état de ma vue, fatiguée par mes longues études sur la lumière, je pourrai continuer ces travaux. J'aurai l'honneur de soumettre les résultats au Bureau des Longitudes.

"J'ajouterai en terminant que j'ai eu l'occasion de continuer aussi mes études sur le spectre de la vapeur d'eau. Le climat de l'Inde, très-humide en ce moment, est très-favorable à ces recherches. Je suis conduit à attribuer au spectre de cette vapeur une importance tous les jours plus grande, l'ensemble de mes études à Paris et ici me conduit à reconnaître une action élective sur l'ensemble des radiations solaires, depuis les rayons obscurs jusqu'aux rayons ultra-violets, bien que dans le violet l'action élective soit beaucoup plus difficile à constater. Ces études formeront l'objet d'une communication séparée."

Southern Survey.

It is with great pleasure that the Council bring before the Society the following report of Mr. Ellery's progress in this important work.

Progress Report of the Melbourne portion of the Southern Survey.

"The zone observations for the portion of the Survey allotted to the Melbourne Observatory were commenced on the 11th April, 1866, as I have already reported.

"It was determined at the outset not to accumulate observations beyond our powers of reduction, and the rule of having one month's work reduced before commencing another has been rigidly adhered to. With the limited staff of this Observatory, and the many other duties devolving on the officers, this course does not admit of very rapid progress being made.

"The work already done comprises the observation and final reduction to the arranged epoch, of all stars down to the 9½ and many to the 10th magnitudes.

Between parallels		150°·00	and	152°·40	N.P.D.	0	m 0	to 24	m 0
,,	,,	152·40		153·20		0	10	22	10
,,	,,	153·20		154·00		11	40	15	40
,,	,,	154·00		154·40		12	10	13	30

"The number of stars observed in these zones and now reduced and tabulated is 20,471.

"The methods of observation have already been reported to the Society; our longer experience with them shows that they are well adapted to this kind of work. The two Chronographs, with

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two hour hands and short conical pendulums, are admirably adapted to zone observations, and give a greater amount of accuracy and far less trouble than usually belongs to such observing. The glass reservoir pens used, however, were for a long time a great source of annoyance from their uncertainty of action, and a two-hours' zone was often spoilt by failures of the pen through plugging of the capillary bore of the nib, or from too free a flow. After many trials of different kinds of pens and pencils for recording, and of the various methods of marking the movements of the magnet armature on paper in the different forms of chronographs and telegraph instruments, I devised a pen which gives so much satisfaction in its operation, and appears so certain and reliable in its action, that I have sent a drawing and description of it as an appendix to this report.

"The mode of reading off the declinations by means of a micrometer dial and count-wheel, which indicate the position of the system of seven declination wires illuminated in a dark field, is, I am convinced, better than using a scale at the focus of the eye-piece. It admits of much more precision and relieves the observer immensely; all the reading-off from the micrometer being done by the assistant."

Spectroscopic Observations of the "Red Prominences" without an Eclipse of the Sun.

Our Fellow, Mr. Lockyer, communicated to the Royal Society, on October 11, 1866, his spectroscopic observations of solar spots, and some deductions from the appearances presented by the absorption lines. At the close of that paper Mr. Lockyer asks, "May not the spectroscope afford evidence of the existence of the red flames which total eclipses have revealed to us in the Sun's atmosphere, although they escape all other methods of observation at other times? and if so, may we not learn something from this of the recent outburst in *Corona?*" This prediction has since been most brilliantly fulfilled by the independent discoveries of M. Janssen and himself.

In January 1867, Mr. Lockyer made an application to the Government Grant Fund, administered by the Royal Society, for a more powerful spectroscope. The application was acceded to, and the construction of the instrument was intrusted to the hands of our late and much-lamented Fellow, Mr. Thomas Cooke. Increasing ill-health and engagement upon Mr. Newall's large object-glass, unfortunately, delayed its construction.

On January 7, 1868, the order was transferred to our Fellow, Mr. Browning, by whom it was finished, and placed in Mr. Lockyer's hands, October 16. At this time we were in possession of the results of the spectroscopic observations of the red flames made at the eclipse of August 18. That the red prominences consisted of luminous gas, giving a spectrum of bright

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lines, was removed from the region of theory and conjecture to that of fact.

On the receipt of his spectroscope, Mr. Lockyer at once applied it to test whether it was sufficiently powerful to render the bright lines of the solar prominences visible by the comparatively greater dispersion of the surrounding light. His success was complete on the first day he gave to the work, viz. October 20th; he was enabled at once to determine that the prominences were built up of hydrogen by the absolute coincidence of two of the bright lines with the Fraunhofer's lines C and F. He was also enabled to establish a fact which may ultimately be of great importance, namely, that the bright line near Fraunhofer's D was not the line D itself, and that it did not absolutely coincide with any absorption line ordinarily present in the solar spectrum. Mr. Lockyer also was enabled to determine at once the shape and dimensions of the prominence he "felt" in his new instrument.

This discovery was announced to the Paris Academy by Mr. De La Rue, a few minutes after a second and more detailed communication from Mr. Lockyer had been placed in the hands of M. Delaunay, the President of that body, a letter was received by him from M. Janssen, who was then in India, announcing that he had also discovered the same method of research some time before, viz., on the day after the eclipse itself.

The number of cases of independent discoveries which have occurred in Science is most remarkable. It would be almost inexplicable were it not for our knowledge of the gradual steps by which each important discovery is led up to, and the number of active minds which are thus directed towards every question as it ripens for solution.

It may have been that had the unfortunate delay not occurred in the completion of his spectroscope, Mr. Lockyer would have been enabled to have anticipated the spectroscopic discoveries made during the eclipse of August 18, 1868. To leave, however, this region of conjecture, we must most warmly congratulate him on the important results which he has already obtained,—results which we hope may prove but an earnest of yet more important observations to follow.

Measures of ζ Herculis.

Mr. Knott has fortunately found a double star *ζ Herculis*, in which the companion falls within the first bright ring. He has thus been enabled to obtain, for the aperture of his telescope, the radius of the first bright ring of the star. The observed value is in most satisfactory accord with its value deduced, for mean rays, from the undulatory theory of light.

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Motion of Sirius in the Line of Sight.

There are three absorption bands in the spectrum of *Sirius* which correspond very closely with the three characteristic lines of luminous hydrogen. Two of these bands have been directly compared with the spectrum of luminous hydrogen. The third is too faint for accurate comparison. The presence of all three characteristic lines and the close agreement of their positions with light of the refrangibilities of luminous hydrogen render it extremely probable that this gas is present in the absorbing atmosphere of the star. Mr. Huggins has, however, found that the brightest hydrogen line, or that corresponding to the solar line F, does not, with the dispersive power at his command, fall centrically within the corresponding absorption band in the spectrum of *Sirius*. The other lines are too faint for such delicate experimental inquiries. We cannot, however, doubt, for the reasons stated, that hydrogen is really present in the absorbing atmosphere of the star.

For comparison with the spectrum of the star, Mr. Huggins has employed hydrogen under different pressures. He has, however, shown that as the pressure is increased, the breadth of the corresponding band, as seen in his instrument, is symmetrically increased. The want of coincidence of the hydrogen line and the corresponding absorption line in the spectrum of *Sirius*, is not therefore a question of pressure. The coincidence of the three magnesium lines, *b*, with the corresponding absorption bands in the Moon's spectrum, appears in Mr. Huggins' apparatus perfect. This would appear to prove that the very great difficulty of insuring a similar refraction through the prisms for the beam from the near terrestrial light and the distant object whose spectrum is under examination, has been successfully overcome.

The want of absolute coincidence of the hydrogen line and the centre of the absorption band in the spectrum of *Sirius*, has been attributed by Mr. Huggins to a change in the refrangibility, of the light arising from the relative radial motions of the Earth and the star. Mr. Huggins' measures have assigned to *Sirius* a velocity of 29·4 miles a second receding in the direction of the line of sight.

Observations of the Spectra of Southern Nebulæ.

Lieut. John Herschel has successfully applied the instruments placed in his hand by the Royal Society for the observation of the Solar Eclipse of August last to an examination of the spectra of the Southern Nebulæ. It is interesting to find that the objects examined by him give, in some cases, a continuous spectrum, and in others a spectrum of one or more of the same bright lines which are seen in the Nebulæ which had been observed in England.

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In the Great Nebula in *Orion* he has detected, in addition to the three lines, a fourth very faint line, more refrangible than F, and probably in the same position as the fourth line seen by Mr. Huggins in 18 H, IV. It is probable that this line may coincide with a line of hydrogen. Lieut. Herschel has also detected a faint continuous spectrum. Lord Rosse and Prof. Winlock have made similar observations, and suspect in addition the presence of some other lines.

The Nebula in Orion.

Two valuable drawings of the Great Nebula in the Sword-hilt of *Orion* have been communicated to the Royal Society, and a third has been published and distributed to Astronomers by Padre Secchi. The most comprehensive of these pictures is that contributed to the Royal Society by the Earl of Rosse, together with a paper, which have been recently published in the *Phil. Trans.* vol. clviii. 1868. The representation of the Nebula is $23\frac{1}{2}$ in. × $25\frac{1}{2}$ in., and comprises about one square degree, namely, 20′ to the north and 40′ to the south of the principal star in the trapezium; and 26′ to the east and 38′ to the west of that star; the nebulous matter extending beyond the star *iota*. This very beautiful picture is the graphic summation of seven years' work, performed with admirable zeal and continuity of purpose by the late noble founder and constructor of the Birr Castle telescopes, and of his staff of assistants; and also that of the present Earl, who has completed the observations commenced by his father. During these seven years it was found necessary to devote the 6-foot and 3-foot reflectors, and occasionally an 18-in. Cassegrainian, mounted equatorially, on every available hour to the work in hand, in order to produce the picture which has been published by the Royal Society. It is printed in fac-simile of the original in such a way that black represents white, but the Council have wisely had printed for distribution a certain number of copies in white ink on a black paper, thus representing the object as in nature.

The second drawing, in which black also represents white, was presented to the Royal Society by Mr. Lassell, in April 1868; it sums up the results of much arduous work performed with his 4-foot Newtonian telescope mounted equatorially, during his sojourn in Malta, extending over a period of rather over three years. This drawing is 25 inches square; it comprises a space of 13′ 20″ to the north and 15′ to the south of the principal star in the trapezium, also 15′ to the east and 13′ 20″ to the west of that star; consequently not quite $\frac{1}{4}$ of a square degree (0·223).

Astronomers have thus at command two drawings of this object by different observers, but made with the aid of instruments comparable as to aperture, although not of precisely similar dimensions. A cursory glance impresses one with the general similarity of the appearances depicted, but a closer inspection of the two pictures renders evident a great disagreement in many points of detail, quite great enough indeed to mislead in respect to any con-

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clusion which might be drawn as to changes having taken place in the configuration of the nebulous matter. This is not to be wondered at, for this object varies so considerably in its aspect not only with instruments of different apertures, but also with the same instrument under different conditions of atmosphere, that it is almost impossible to state with any degree of certainty whether any physical changes have taken place in the several parts of the Nebula during the period in which it has been carefully scrutinised. The importance to future Astronomers of having drawings which have been made at about the same epoch by well-practised observers, with the aid of competent and very similar instruments, cannot therefore be overrated; and it is to be hoped on this account that the Royal Society will publish Mr. Lassell's drawing.

Padre Secchi's picture represents a space comprised within the limits of 10′ to the north, 12′ to the south, 10′ to the east, and 12′ to the west of the principal star of the trapezium; or 0·134 square degree; it is also printed in black for white. On taking into consideration the comparatively very small aperture of the instrument employed it exhibits a great amount of detail; and will have its value in the future, more especially as presenting some such general aspect of this nebula as the valuable picture which a finder to large telescope offers of a comet to be scrutinised for minute details in its larger companion. It differs, however, as might be expected very considerably from both Lord Rosse's and Mr. Lassell's drawings.

It is to be expected that now that the Melbourne 4-foot Cassegrainian has arrived at its destination, and is being erected, another record will be made of the aspect of this vast assemblage of nebulous and stellar matter, under the peculiarly favourable circumstances which the handiness of the instrument and the advantageous position of the observing station present.

When one reflects on the vast dimensions of the Nebula of *Orion*, it is difficult to realise the probability of changes occurring to such an extent in the distribution of the nebulous matter as to be recognisable at very short intervals; it is, therefore, as a basis for comparison with observations made by Astronomers some centuries hence, that the pictures to which we have drawn attention must be regarded as of the highest value; for in these remote periods possibly real changes will have occurred which cannot be confounded with the apparent changes dependent on variations of atmospheric conditions.

The Variable Star η Argûs and its Surrounding Nebula.

The papers which Mr. Abbott has communicated to the Society on this subject appear to indicate changes in the distribution of the nebulous matter around *η Argûs* of the most rapid and astounding character.

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Sir John Herschel, in the *Notice* for June, vol. xxviii., has pointed out his inability to identify any of the stars near η *Argûs*, in Mr. Abbott's drawing, and expressed the strongest wish that the point thus raised of the reality of the changes in the Nebula around η *Argûs* should be settled definitely and at once. The wish thus expressed was not likely to remain disregarded.

A drawing of the Nebula has already been presented to the Society by Lieut. Herschel. In this drawing the star η *Argûs* is represented as quite within the region of nebulous matter. Lieut. Herschel's drawing agrees far more closely with his father's drawing of 1834–7 than with either of Mr. Abbott's drawings. It is, however, desirable that the Nebula should be examined under more favourable circumstances than those at the command of Lieut. Herschel.

November Meteors.

The shower of last November assumed almost unexpected proportions. Unfavourable weather interfered to a considerable extent with its observation in England. A very considerable number of meteors were, however, observed, and the position of the radiant fixed with accuracy at the Greenwich Observatory. Observers in the south of Europe, at the Cape of Good Hope, in Australia, and on the American continent, were more fortunate. The mass of materials thus collected proves the shower to have been on a most extensive scale. The succession of large November showers for at least four successive years, would appear to prove that the arc of the orbit over which considerable condensation exists is far more extensive than was formerly supposed.

The Approaching Transits of Venus of 1874 *and* 1882.

The Society, and all interested in the advancement of our noble science, are indebted to the Astronomer Royal for an elaborate discussion of the best means for utilising the approaching Transits of *Venus* in 1874 and 1882.

The method of differences of observed durations will not be the best available for the determination of Solar Parallax in 1874, and probably also not in that of 1882.

It will thus be necessary to determine with extreme accuracy the longitudes of the different stations. This necessity calls for immediate action. The Astronomer Royal has most carefully pointed out in his paper the stations which are best adapted for the location of the observers. It is one of the many advantages which we may expect to derive incidentally from the proposed expeditions, that we shall be thus driven to determine with extreme accuracy the position of so many distant stations, which will afterwards be available as zeros of longitude.

Mr. Warren De La Rue has urged the advisability of supplementing the eye-observations by photography. He is of opin-

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ion that a value of the solar parallax of considerable weight may be thus obtained. His great experience and proved skill in the deduction of accurate results from photographic registers entitle his opinion to the greatest consideration.

The Astronomer Royal has proposed that altazimuths should be employed in the determination of the longitudes of the stations. Results of very much the same value as those to be expected from photography might probably be obtained without any additional expense, or interfering in the slightest degree with any other results by simply observing throughout the transits, with those altazimuths, the position of the planet with reference to the Sun's limbs.

The Astronomer Royal's application for the necessary funds is sure to meet with that liberal consideration which the British Government has ever shown towards all well-considered schemes for the advancement of science.

Discovery of Minor Planets.

Twelve Minor Planets have been discovered during the year 1868. This is the greatest number added to the list of planets in any single year. It will be seen from the tabular statement below that the diligence and zeal of Professor Watson of Ann Arbor, Michigan, have been rewarded by the discovery of no less than six out of the twelve of these minute members of the solar system. The date of each discovery is as follows:—

Planet.	Discoverer.	Date of Discovery.
Ægle	Coggia	1868, February 17
Clotho	Tempel	,, February 17
Ianthe	C. H. F. Peters	,, April 18
(99)	Borelly	,, May 29
Hecate	Watson	,, July 11
Helena	Watson	,, August 16
Miriam	C. H. F. Peters	,, August 22
(103)	Watson	,, September 7
(104)	Watson	,, September 13
(105)	Watson	,, September 16
(106)	Watson	,, October 10
Camilla	Pogson	,, November 17

A Comet, Comet I. 1868, was discovered by Dr. Winnecke on June 13.

Brorsen's and Encke's periodical Comets have been observed at their respective oppositions.

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Spectrum of Comet II. 1868.

The spectrum of this faint Comet has been examined by Mr. Huggins.

There appears a very close agreement between its spectrum and that of the vapour of carbon, when obtained by the decomposition of olefiant gas and some other compounds of carbon.

Rediscussion of the Observations of the Transit of Venus, 1769.

Our Secretary, Mr. Stone, has done an important service for Astronomy by an independent discussion of the observations of the Transit of *Venus*, 1769. Skilfully applying to the descriptions of the observers an interpretation founded on the optical phenomena which necessarily attend the transit of a small dark body over the Sun, he has shown that the observations of the different observers, so far from being discordant, are in close agreement with each other, and furnish a value of the Solar Parallax which is almost identical with the new value which results from the observations of *Mars*. A more complete account of this investigation, and of Mr. Stone's other contributions to our science, will be found in the President's statement of the reasons for the award by the Council of the Gold Medal to that gentleman.

Reference in this place should be made, however, to some other papers which Mr. Stone has presented to the Society during the present year. One of these contains an investigation of the principles on which observations, which differ considerably from the general average, should be dealt with. He has deduced therefrom for different values of the average accuracy of the observer, and with certain assumed probable errors, the limits beyond which all discordant observations should be rejected.

In another paper, from an independent determination, he has found for the Moon's mass the value—

$$\frac{\text{Moon's Mass}}{\text{Earth's Mass}} = \frac{1}{81{\cdot}36}$$

Two papers are devoted to a determination of the constant of nutation, from observations in N.P.D. of *Polaris*, *Cephei* 51, and δ *Ursæ Minoris*, and in R.A. of *Polaris*, made at Greenwich from 1851 to 1867.

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Communications to the Society from February 1868 *to February* 1869.

1868.

Mar. 13. Additional Identification of Double Stars in Sir William Herschel's Catalogue. Sir J. Herschel.
Observations of Minor Planets. Dr. R. Luther.
On the Lunar Crater *Linné*. Rev. T. W. Webb.
Sur les Comètes, 1857, III. and V., and 1867, III. M. Hoek.
On the Planetary Nebula 45 H IV. *Geminorum*. Rev. H. C. Key.
Sur les Phénomènes qu'un essaim de Météores très étendu venant de l'Espace présente après son entrée dans le Système Solaire. M. Hoek.

April 8. On the Lunar Crater *Linné*. Mr. Crossley.
On some Markings seen on *Venus*. Mr. Browning.
Catalogue of Known and Suspected Binary Stars. Mr. Chambers.
On the Employment of the Table given in the *Nautical Almanac* showing the correction required on account of second differences in finding the Greenwich Time corresponding to a reduced Lunar Distance. Capt. Shadwell.
On the Lustre of *Venus*. Mr. Weston.
Description of the Zenith Telescope of the United States Survey. Mr. Davison.
On the Rejection of Discordant Observations. Mr. Stone.
The Great Nebula of *Orion*. Father Secchi.
Proposal for a New Star Atlas. Mr. Proctor.
Lunar Crater *Linné*. Capt. Noble.

May 8. Occultation of γ *Tauri*. Mr. Joynson.
Lunar Crater *Linné*. Rev. T. W. Webb.
On the Variability of η *Argûs* and the surrounding Nebula. Mr. Abbott.
Eclipses of *Jupiter's* Satellites. Mr. Tebbutt, jun.
On the Stellar Longitudes assigned by Ptolemy. Major Drayson.
On the Lunar Crater *Linné*. Mr. Prince.
Remarks on the Name of the Star ξ *Scorpii* (= Fl. 51 *Libræ*). Mr. Brothers.
On a new Driving Clock for Equatoreals. Mr. Cooke.
Ephemeris of Brorsen's Comet. Dr. Bruhns.
Questions relative to the Invention of the Achromatic Telescope. Mr. Wackerbarth.
Note on *Linné*. Mr. Birt.
Catalogue of Spectra of New Stars. Father Secchi.

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List of Stars to be Occulted about the time of the Great Eclipse, August, 1868. M. Ragonatha Chary.

On a Persistent Marking on *Jupiter*. Mr. Browning.

June 12. On the Variable Star η *Argûs* and the surrounding Nebula. Sir J. Herschel.

Observations of the Lunar Crater IV. A. α 17 IV. A. 339. Mr. Birt.

Determination of the Constant of Nutation from the Observations in N.P.D. of *Polaris*, *Cephei* 51 and β *Ursæ Minoris*, made with the Greenwich Transit Circle, 1851 to 1865. Mr. Stone.

Nov. 13. On the Nebular Hypothesis and the Approximate Commensurability of the Planetary Periods. Mr. Kirkwood.

Re-discussion of the Observations of the Transit of *Venus*, 1769. Mr. Stone.

Observations of the Total Eclipse of August 18, 1868. Mr. Reed.

Ditto ditto Mr. Tebbutt, jun.

Observations of the Transit of *Mercury*, November 4, 1868. Prof. Chevallier.

Ditto ditto Mr. G. Williams.

On the Discovery of the Variable Star T *Coronæ*. Mr. Barker.

Description of an Automatic Transit Instrument. Mr. Kincaid.

Occultations observed at Durham. Prof. Chevallier.

On the Early History of the Achromatic Telescope. Mr. Chambers.

Transit of *Mercury*. Mr. Prince.

Ditto ditto Prof. C. P. Smyth.

Description of Drawings of *Mars*. Mr. Joynson.

Occultations of Stars of *Jupiter's* Satellites. Mr. Joynson.

Heliographical Elements of Sun-spots, August 18, 1868. Mr. Loewy.

Transit of *Mercury*. Mr. Lassell.

Field Notes of Zenith Telescope. Mr. Davidson.

On the Appearance of *Mercury* at its Transit, Nov. 5, 1868. Mr. Huggins.

On a possible Method of Viewing the Red Flames without an Eclipse. Mr. Huggins.

On the Lunar Crater *Linné*. Mr. Birt.

Letter to Rev. C. Pritchard. Sir J. Herschel.

Observations at Greenwich of the Egress of the Planet *Mercury* from the Sun's Disk, Nov. 5, 1868. The Astronomer Royal.

On a proposed Form of Spirit Level. Major Tennant.

Remarks on Mr. Stone's Paper on the Transit of *Venus*, 1769. Mr. Newcomb.

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Reply to on Mr. Newcomb's Remarks on his Paper on the Transit of *Venus*, 1769. Mr. Stone.
Some Remarks and Suggestions arising from the Observations of the Transit of *Mercury*, 1868, Nov. 4. Mr. Stone.
Determination of the Constant of Nutation from Right Ascension Observations of *Polaris*. Mr. Stone.
On Aboul Hassan's Catalogue of Stars. Mr. Stone.
The anticipated Weather of 1869. Mr. Best.
On the Cause of the Difference of Time between the Mornings and the Evenings. Mr. Best.
Transit of *Mercury*. Mr. Penrose.
Ditto ditto Capt. Noble.
Ditto ditto (Photographs). Rev. Dr. Selwyn.
Reports on Solar Eclipse, Aug. 1868. Mr. Smith.
Note on his Catalogue of Binary Stars. Mr. Chambers.
Description of a new Chronograph Pen. Mr. Ellery.
Transit of *Mercury*. Mr. Buckingham.
Dec. 11. Meteors of November 1868. Mr. Kirkwood.
Measures of ζ *Herculis*. Mr. Knott.
Transit of *Mercury*. Mr. Knott.
Ditto ditto Mr. Browning.
Further Paper on an Automatic Transit Instrument. Mr. Kincaid.
Elements of newly discovered Asteroids. Mr. Watson.
Observations of the Solar Eclipse, Aug. 1868. M. Cheetry.
Ditto ditto ditto Maj. Tennant.
On a Defective Vision. Major Tennant.
Ephemeris for Observations of the Physical Features of the Planet *Mars* during the Opposition of 1869. Mr. Marth.
Observations of the Meteoric Shower of Nov. 13–14, 1868. Prof. Grant.
Preparations for Observing the Transits of *Venus*, 1874, 1882. The Astronomer Royal.
On the Observations of the Transits of *Venus* by Photography. Mr. De La Rue.
Observations of Meteors, 1868, Nov. 14. Admiral Sir E. Belcher.
1869. Transit of *Mercury*. Mr. Talmage.
Jan. 8. On the Physical Constitution of the Sun. Commander Ashe.
Comparison of Sun-spot Observations made in Dessau and Kew Observatory. Messrs. De La Rue, Stewart, and Loewy.
On the Relation of the Luminous Prominences to the Faculæ of the Sun. Mr. Brayley.
Transit of *Mercury*. Mr. Todd.
Description of the Great Nebula round η *Argûs*. Sir J. Herschel.

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Note on Mr. Huggins' Paper, "On a Possible Method of Viewing the Red Flames without an Eclipse." Mr. Lockyer.
Meteors of Dec. 11, 1868. Mr. Finlayson.
Letter to Admiral Manners. Sir J. Herschel.

List of Public Institutions and of Persons who have contributed to the Society's Library, &c. since the last Anniversary.

Her Majesty's Government.
The Lords Commissioners of the Admiralty.
The Secretary of State for War.
Royal Society of London.
Royal Asiatic Society.
Royal Geographical Society.
Royal Institution.
Royal United Service Institution.
Royal Scottish Society of Arts.
Cambridge Philosophical Society.
Geological Society.
Photographic Society.
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Society of Physics, Berlin.
Society of Sciences, Leipsig.
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Academy of Sciences, Montpelier.
Instituto-Tecnico, Palermo.
Astronomische Gesellschaft, Leipsig.
American Academy of Arts and Sciences.
American Philosophical Society.
American Association.
American Academy of Natural Sciences.
United States Coast Survey.
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ADDRESS

Delivered by the President, Admiral Manners, on Presenting the Gold Medal of the Society to Mr. E. J. Stone.

Gentlemen, — The pleasing duty now devolves upon me, as President of this Society, to give effect to the resolution which you have adopted with such cordial unanimity by presenting to Mr. Edward James Stone the usual form of testimonial, which, in accordance with the bye-laws, is awarded annually by the Society as a recognition on its part of special services rendered in promoting the science of Astronomy.

Mr. Stone, as many of you are aware, has for nearly nine years occupied the responsible position of First Assistant at the Royal Observatory, Greenwich. He succeeded in this post to a gentleman, a valued member of our Society, who for many years had discharged the duties of his office in a manner at once eminently honourable to himself and advantageous to the interests of the renowned Institution with which he was associated. I need scarcely inform you that I refer to the able Director of the Radcliffe Observatory, Oxford.

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Educated at the University within the ancient Halls of which Newton completed his academical studies, Mr. Stone entered upon his duties at the Royal Observatory with a mind thoroughly trained in a course of instruction which was eminently adapted to the career in life he had chosen.

I have reason to believe that, during the period of his connexion with the Royal Observatory, Mr. Stone has conducted himself throughout in such a manner as to earn the unqualified approbation of the Chief of that Establishment. With this, however, we have nothing to do on the present occasion.

The Society, except under circumstances of very rare occurrence, takes no cognizance of labours, however honourable and useful, which may have been performed merely in the ordinary course of official life.

We are now met, Gentlemen, to affix the seal of our recognition to services which have been rendered to Astronomy during leisure hours exclusively, which have been prompted solely by that ardent thirst for knowledge and that irrepressible longing after truth which constitute one of the distinguishing features of our common humanity.

I shall now proceed to state to you, in as concise terms as circumstances will admit of, the grounds upon which the Council unanimously resolved that the Medal of the Society should be awarded this year to Mr. Stone.

During the last few years Mr. Stone has contributed to the Society a great number of important papers on different subjects of Astronomy, which have been published either in the *Memoirs* or in the *Monthly Notices* of the Society.

Any person who gives those papers an attentive perusal will not fail to discover that they have for their leading object the investigation of improved values of the fundamental constants of astronomy by a searching discussion of large masses of trustworthy observations.

In the prosecution of his researches Mr. Stone has availed himself chiefly of the stores of valuable observations which have accumulated at the Royal Observatory, Greenwich, more especially during the incumbency of the present illustrious Director of the Institution.

I shall commence my brief review of Mr. Stone's astronomical labours with some account of his researches on the Solar Parallax.

The ancient astronomers were absolutely unacquainted with even an approximate value of the Solar Parallax. Their observations were of too rudimentary a character to exhibit any trace of an element of such delicate minuteness. The same ignorance prevailed in modern times till towards the close of the seventeenth century, when the application of the telescope to divided instruments and the invention of methods for ascertaining the errors of adjustment of instruments placed in the meridian, resulted in effecting a complete revolution in practical astronomy.

It was then only that the results of astronomical observation

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acquired such a degree of precision as to hold out any hope of detecting a change in the apparent position of the Sun, or any of the planets, depending upon the inclination of the line of vision of the object with respect to the imaginary line connecting the observer with the centre of the Earth. It is well known that in the case of the Sun the determination of such a change of apparent position by direct observations of that body is beset in practice with insurmountable difficulties. Fortunately the planets *Venus* and *Mars*, which revolve, the one immediately within, the other immediately beyond the Earth's orbit, supply the astronomer with excellent methods for attacking the problem of the Solar Parallax by an indirect course of procedure.

I shall first refer to the case of *Mars*.

The orbit of this planet, besides being very excentric, is so situated with respect to the Earth's orbit that the perihelion of the one is turned very nearly towards the aphelion of the other. The result is that, in the course of successive intervals of seven or eight synodic revolutions, the planet when at the perihelion approaches very near to the Earth, and, in consequence, occupies a position which is eminently favourable for the investigation of its parallax.

We owe to Dominique Cassini the earliest determination of the Solar Parallax by a method based upon sound principles.

By a comparison of meridian observations of *Mars* made in 1671 by Richer, at Cayenne, in South America, with corresponding observations of the planet made in Paris, he obtained $9''{\cdot}5$ as the most probable value of the Solar Parallax.

This was an immense step in advance, for, in all previous investigations the Solar Parallax was estimated in minutes. Several subsequent attempts to determine the Solar Parallax were made at different times down to the middle of the last century on the occasion of the near approach of *Mars* to the Earth, but in hardly any instance was the result so good as that which Cassini originally deduced from his researches on the subject.

The method for determining the Solar Parallax to which I have just referred was now about to be supplanted for a time by a new method founded upon observations of the transits of *Venus* over the Sun's disk. The peculiar advantages of this method had been forcibly pointed out by Halley.

The transits of 1761 and 1769 were close at hand, and the astronomers of the day were so far successful in impressing the public mind with their supreme importance, that the principal Governments of Europe sent out expeditions to different parts of the world for the purpose of making accurate observations of them. The results of those expeditions are well known. The transit of 1761 was from various causes imperfectly observed. The operations for observing the transit of 1769 were, on the other hand, decidedly successful. Shortly afterwards, various results for the Solar Parallax were obtained from the observations,

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fluctuating between 8″·6 and 8″·8; but, as no method of discussion by a comprehensive and systematic process had been employed by any astronomer in such investigations, the definitive result of the observations could not be said to have been determined. This object, however, was finally effected by Encke, in his treatise, "Der Venusdurchgang von 1769," published at Gotha in 1824. The celebrated German astronomer determined the Solar Parallax to be 8″·5776 as the definitive result of his researches; and this value has since been universally adopted by astronomers till within the last few years.

In 1836, Professor Henderson determined the value of the Solar Parallax by a comparison of a few observations of *Mars* made at the Cape of Good Hope in 1832, with the corresponding observations made in the northern hemisphere at Greenwich, Cambridge, and Altona.

He obtained 9″·028 as the result of his researches, but the materials of his investigation were too scanty, and the residual errors were too discordant to affect the trustworthiness of the result which Encke had already deduced from the transit of *Venus* in 1769.

At the meeting of this Society, held in May 1857, the Astronomer Royal directed the attention of the Society to the great importance of determining the Solar Parallax by means of observations of *Mars* made at the approaching oppositions of 1860 and 1862.

This may now be the proper place to state that suspicions had already begun to be entertained respecting the accuracy of the value of the Solar Parallax which Encke had deduced from the transit of *Venus* in 1769.

The earliest indication of the necessity of an alteration of the received value is to be found in a letter from Professor Hansen to the Astronomer Royal, which is published in the *Monthly Notices* for November 1854 (vol. xv.). In this letter Professor Hansen states that the coefficient of the parallactic equation in the Moon's longitude, as determined by the Greenwich Observations, necessarily implies a greater value of the Solar Parallax than that which had been deduced from the transit of *Venus*, and he adds that the Dorpat Observations led to a similar conclusion.

The results of M. Le Verrier's researches shortly afterwards indicated the necessity of a similar increase of Encke's value of the Solar Parallax. In the *Annales de l'Observatoire Impérial de Paris*, vol. iv. 1858, he has determined from observation the coefficient of the lunar inequality in the Earth's longitude, and hence obtains 8″·95 for the value of the Solar Parallax. This result he has incorporated in his Solar Tables.

In vol. vi. of the same work the researches of M. Le Verrier on the movements of *Mars* and *Venus* conduct him to a similar conclusion as regards the necessity of increasing the received value of the Solar Parallax.

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It may be mentioned also that about the same time the late Mons. Foucault obtained, by an ingenious arrangement, a measure of the velocity of light which necessarily implied a diminution of the radius of the Earth's orbit.

Such, then, Gentlemen, was the state of the question of the Solar Parallax previous to the opposition of *Mars* in 1862.

The opposition of 1860 had passed without attracting any special notice, the attention of astronomers having been mainly occupied with the total eclipse of the Sun, which happened in the month of July of that year. Fortunately, on the other hand, the opposition of 1862 was well observed at several important stations both in the northern and the southern hemispheres.

The principle of the method which was generally agreed upon as best adapted for the determination of the Solar Parallax in this instance, consisted in observing with meridional instruments the differences of declination between the planet and certain comparison stars which lay in the vicinity of its apparent path. Upon the basis of a considerable portion of the materials thus formed, Mr. Stone investigated the Solar Parallax in a paper which has been published in the *Memoirs* of the Society.

I cannot do better than quote his words with reference to the advantages which the method offers for effecting the object in view: — "The peculiar advantages of the method adopted appear to be that the observations can be made with the instruments of fixed observatories by observers in the ordinary routine of their work, the results obtained are so far removed from the quantities immediately observed that no knowledge of the results expected to be obtained can influence the observers. The number of observers can be sufficiently multiplied to diminish considerably every injurious effect of personality, by taking stars properly distributed above and below, and preceding and following *Mars* the effects of errors of the refraction-correction, at least in a mean result, can be destroyed; the effects of any division errrors likely to exist uncorrected in our modern instruments can be destroyed in the same way; and lastly, the observations may be indefinitely multiplied, and a continuous approximation carried on by availing ourselves of the favourable oppositions of *Mars*."

The materials which constitute the groundwork of Mr. Stone's investigation consist of observations of *Mars* and neighbouring stars made at Greenwich, the Cape of Good Hope, and Williamstown in Victoria, during the interval included between 1862, August 25, and November 23.

The results of his researches are exhibited in a system of nine tables. The three first tables contain the reduced north polar distances of the planet as determined at the three observatories. In the fourth and fifth tables the observations of the comparison stars made at the Cape of Good Hope and Williamstown are compared with the corresponding Greenwich errors and appropriate corrections hence determined.

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In the sixth table the error of the Ephemeris of the planet is investigated by the aid of the Greenwich Observations.

In the seventh table the Greenwich and Cape Observations are combined together, and the Sun's equatorial horizontal parallax computed for each separate day of observation, each result having a certain weight attached to it.

In the eighth table the Greenwich and Williamstown observations are similarly combined together, and the resulting parallaxes exhibited with their appropriate weights.

In the ninth and last table the parallax for each day of observation, with its proper weight attached to it, is computed from a combination of the Greenwich, Cape of Good Hope, and Williamstown Observations, and the definitive value of the Solar Parallax is deduced. This Mr. Stone finds to be $8''\cdot943$, with a probable error of $0''\cdot051$.

It is unnecessary for me to expatiate upon the merits of this excellent paper.

The resulting value of the Solar Parallax obtained by Mr. Stone on this occasion presented a satisfactory agreement with the values of the same element which had been recently derived from other sources, and tended to strengthen the suspicion already entertained respecting the result which Encke had deduced from the observations of the Transits of *Venus*.

The next determination of the Solar Parallax by Mr. Stone is contained in a paper on the "Value of the Co-efficient of the Parallactic Inequality in the Moon's Longitude, determined by means of the Observations of the Moon made at Greenwich between the years 1848 and 1866."

The materials of this discussion consist of 2075 observations of the Moon.

The resulting value of the co-efficient of the parallactic equation is found to be $125''\cdot36$, and hence the value of the Solar Parallax is determined to be $8''\cdot850$.

I should mention here that Professor Hansen, in a communication to the Society which appears in vol. xxiv. of the *Monthly Notices*, had assigned $8''\cdot916$ as the value of the Solar Parallax derivable from his researhes on the lunar theory.

In 1864 Mr. Powalky subjected the observations of the transits of *Venus* to a fresh discussion, and determined the Solar Parallax to be $8''\cdot832$.

This discussion, which is evidently the result of very much labour, is based upon more accurate determinations of longitude than those available in Encke's time.

The rejection of many observations at the more important stations for reasons which do not appear to be warranted by any legitimate principles, and the imperfect manner in which the very few durations employed are represented in the residuals detract much from the value which would otherwise attach to this result. The weight of Encke's value would scarcely be said to be affected by this discussion.

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That value still continued to put a veto on any acceptance of the values which resulted from the other methods which had applied to the determination of the Solar Parallax.

Mr. Stone's rediscussion of the Transit of *Venus* of 1769 is published in the Supplementary number of the twenty-eighth volume of the *Monthly Notices*. The materials of his researches consist of ten complete observations of the Transit made at Wardhus, Hudson's Bay, Kola, St. Joseph in California, and Otaheite.

Incomplete observations were excluded in consequence of the uncertainty which in general existed respecting the longitudes of the several stations, and the sufficiency of the observed durations for an accurate determination of the parallax.

Mr. Stone uses both apparent and real internal contacts. Corresponding to each phase, or rather to each mean phase of apparent contact and real contact, he introduces an unknown quantity. These unknown quantities are determined with the parallax correction from the ten equations of condition. The two corrections corresponding to apparent and mean phase are not, however, independent; their difference must be equal to twice the time between the mean phase of apparent and real contacts.

The resulting value of Solar Parallax is $8''{\cdot}91$, with a probable error of about $0''{\cdot}03$.

This result presents a most satisfactory agreement with the results of recent researches on the same subject derived from other sources.

Furthermore, the differences between the observed and computed durations are reduced unequivocally within the limit of the errors of observation. Thus, for example, in the case of the observations of Father Hell at Wardhus, which proved such a stumbling-block in previous researches, the residual error is reduced to $1^s{\cdot}7$. In point of fact the outstanding discordance between the observed and computed duration is in every case below 6^s.

Now, with reference to this investigation of Mr. Stone's, I would remark that it includes every complete observation from internal contacts of the transit, and yet, notwithstanding this circumstance, all the observed durations are represented with a degree of accuracy which is, beyond all doubt, within the limit of the probable errors of observation. The author has effected this object by simple interpretation of the words of the observer, inferring from his language and from the accompanying circumstances of the case whether the phenomenon noted by him referred to an apparent contact or a real contact.

This is all the liberty that Mr. Stone has allowed himself, and even this liberty has not been available except in those cases where the observer's own words are ambiguous. He has rejected nothing.

It is true that the method adopted by Mr. Stone involves the introduction into his researches of an unknown quantity corresponding to each observed phase. This is, however, equivalent to the arbitrary selection of real contact observations which had

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always been previously done. In Mr. Stone's investigation, as I have already stated, the two corrections thus introduced are not independent but subject to a necessary condition. This condition is satisfied. There could not be a stronger proof than this of the legitimacy of the method pursued by Mr. Stone, according to which he introduced into his researches apparent or real contacts indiscriminately, according as the one or the other was noted by the observer.

In his paper on this subject Mr. Stone has given a faithful citation of the original observations, by which any person may satisfy himself respecting the interpretation which the author has put upon the words employed by each observer.

I have carefully perused those records, and I feel bound to state that, taking due note of the effects of irradiation and of the distinction existing between apparent and real contacts, the interpretation put upon the words of the original by Mr. Stone appears to me to be their true and simple meaning, and to be therefore the only legitimate interpretation which they can admit of.

By this important investigation, then, Mr. Stone has earned for himself the gratitude of Astronomers of all countries. He has shown beyond all doubt that the method pursued by his illustrious countryman Halley, when fairly treated, is capable of furnishing a value of the Solar Parallax commensurate in precision with the expectations formed of it.

But this is not all. Mr. Stone, by his researches in this instance, has wiped from Astronomy a reproach, which did not indeed legitimately attach to it, but which only one of those intellectual triumphs which from time to time have adorned the annals of our science was capable of extirpating.

I would mention further that by thus showing how completely the resulting value of the Solar Parallax depends upon apparently so slight a cause as misconception of obvious words respecting real and apparent contacts, he has done invaluable service in preparing the minds of Astronomers for the proper observation of the approaching transits of 1874 and 1882, and saved Astronomy from the possibility of a compromising and expensive failure.

We owe to Mr. Stone an important paper on the Constant of Lunar Parallax. His investigation is founded on a comparison of north polar distances of the Moon, determined at Greenwich during the years 1856–61, with corresponding places deduced from observations made at the Cape of Good Hope.

The aggregate number of observations is 239. As it is well known that considerable personality exists in the measurement of the disks of the various bodies of the planetary system, the author adopted the expedient of discussing separately the north and south limbs of the Moon.

The results to which he was conducted by this mode of procedure fully justified the considerations by which he was guided. He ascertained, in fact, that the correction to the constant of lunar parallax resulting from observations of the two limbs was sensibly

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different. By a judicious combination of the separate results, however, he obtained a definitive correction, which, when applied to the theoretical value of the constant, conducted him to a value represented by 3422″·707, with a probable error of 0″·049.

The amount of the correction deduced in this instance was small, but the necessity of its application was plainly indicated by the observations, and it presents a perfect agreement with the result of a similar investigation due to Mr. Breen which is published in vol. xxxii. of the *Memoirs of the Society*.

I have now to make a few remarks on certain researches by Mr. Stone bearing more directly upon the accurate determination of the places of the stars. I would refer in the first place to a paper by him "On the Accuracy of the Fundamental Right Ascensions of the Greenwich Seven-year Catalogue for 1860."

It is well known that during the incumbency of the present Director there have emanated from the Royal Observatory, Greenwich, three Star Catalogues of fundamental importance.

First, we have the Twelve-year Catalogue of 2156 Stars, epoch 1845; then we have the Six-year Catalogue of 1576 Stars, epoch 1850; and, finally, we have the Seven-year Catalogue of 2022 Stars, epoch 1860.

Now a consideration of these Catalogues suggests two questions which it is most desirable to elucidate. In the first place, are there any grounds for supposing that the ultimate results may be affected by systematic errors, whether arising from irregularities in the rate of the clock or in the instrumental corrections, which the methods of reduction actually pursued have been incapable of eliminating?

And, secondly, is the process of reduction and successive cataloguing such as to lead to progressive accuracy as regards the resulting star-places?

Now, with reference to these points, Mr. Stone remarks that the method employed at Greenwich for determining the mean right ascensions of the fundamental stars has a tendency to affect the results with a common error, the elimination of which may be effected by observations of the Sun at the equinoxes. But what must be especially guarded against is the introduction of systematic errors, which neither observations of the Sun nor a repetition of the observations of the stars themselves can have the slightest tendency to eradicate.

Mr. Stone's method of investigation is accordingly based upon a separation of the assumed right ascension of the stars from any systematic errors with which they might be affected in the process of reduction. With a view to this object, he selects four stars of reference situated at nearly six hours of right ascension apart from each other, *γ Pegasi*, *Pollux*, *Spica*, and *α Aquilæ*, and then assuming the mean right ascension of those stars as given in the Seven-year Catalogue with unknown corrections applied to them, he forms a system of equations involving the *relative* corrections of the mean right ascensions of every combination of the reference

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stars, taken two and two, for each of the seven years upon which the Catalogue is based, the relative corrections for each pair of stars being determined by means of the intermediate fundamental stars.

Finally, by the combination of the equations for the different years, he determines the definitive value of the *relative* corrections of the mean right ascensions of the four representative stars.

The result of a comparison of the relative corrections is highly satisfactory, inasmuch as it shows clearly the absence of any systematic errors of appreciable magnitude depending upon the right ascensions of the stars. Mr. Stone shows, furthermore, in the same paper, that the outstanding relative corrections of the Seven-year and Six-year Catalogues are considerably smaller than the relative corrections of the Seven-year and Twelve-year Catalogues.

I have been compelled to advert only in very brief terms to this important paper of Mr. Stone's, but I feel assured that any person who has given attention to it cannot fail to be impressed with the ability and skill with which the author has discussed the immense mass of observations constituting the groundwork of his researches.

The subject of atmospheric refraction is one which has occupied the attention of some of the most eminent astronomers and mathematicians of Europe from the time of Cassini and Newton down to the present day. The refractions now chiefly used by astronomers are those of Bessel, which were originally published in the *Fundamenta Astronomiæ*, and afterwards in a somewhat modified form in the *Tabulæ Regiomontanæ*. The theory, as is well known, is the same in both works, the only difference being that from the zenith to 85° the refractions in Bessel's later work, the *Regiomontanæ*, exceed in a slight degree those of the *Fundamenta*.

At Greenwich it has been the practice since 1857 to employ a still further modification of Bessel's refractions. From the zenith to 82° the mean refractions are those of the *Tabulæ Regiomontanæ* unaltered; from 82° to 85°, the refractions are those of the *Tabulæ Regiomontanæ* slightly diminished; while from 85° to the horizon, the mean refractions are those of the *Fundamenta* slightly diminished also. This arrangement was the result of an elaborate investigation of refraction by Mr. Main, founded partly on observations of Greenwich Circumpolar stars, and partly also on observations of South stars common to Greenwich and the Cape of Good Hope. No astronomer could be more deeply impressed than Mr. Main was with the want of symmetry which this arrangement exhibited, and, while recommending its adoption as indispensable in the existing state of matters, he regarded it merely as a provisional expedient to be supplanted at some future time by the results of a more advanced theory.

One of the objects proposed by Mr. Stone is to show that the

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refractions of the *Tabulæ Regiomontanæ* from the zenith to 85° are somewhat too great.

The materials of his investigation consist partly of observations of Greenwich Circumpolar Stars, and partly of observations of stars common to Greenwich and Melbourne. The results, in so far as the latter place is concerned, refer exclusively to the years 1863, 1864, and 1865.

First, considering the case of the Greenwich Circumpolar stars, he arranges his materials in five groups, to each of which he assigns a weight according to the number of observations. He then forms an equal number of equations of condition involving a correction to the *Tabulæ Regiomontanæ*, and a correction to the co-latitude of Greenwich, and, solving them by the method of least squares, he obtains the values of the unknown quantities.

The result of this process shows that the refractions of the *Tabulæ Regiomontànæ*, down to 85° of zenith distance, are too great by a quantity of sensible magnitude, as is seen in the very considerable reduction of the residual quantities of the equations, arising from the substitution of the numerical values of the corrections.

Now Mr. Stone shows that the result of that correction is tantamount to diminishing the mean refractions of the *Fundamenta* in the proportion of 0·99797 to 1.

But at present the refractions below 85° employed at Greenwich are those of the *Fundamenta* diminished in the proportion of 0·99848 to 1.

Mr. Stone accordingly concludes that if the mean refractions of the *Fundamenta* diminished in the proportion of 0·99797 to 1 were adopted for Greenwich, there would hence result the advantage of being able to compute the refractions from the zenith to the horizon with one set of tables.

Mr. Stone then proceeds to investigate the correction for the refractions of Melbourne by means of observations of Circumpolar stars, and finally the corrections applicable to Greenwich and Melbourne respectively, by means of the observations of stars common to both Observatories. But it would occupy too much of your time if I were to enter into any further details. I may state briefly that the author obtains for the Greenwich refractions a correction the same in sign as that which he had previously derived from his discussion of the Greenwich circumpolar stars, although a correction of rather less magnitude.

With respect to the Melbourne refractions he obtains corrections agreeing in sign with the corrections to the Greenwich refractions, but differing from them in magnitude, and also differing sensibly for North and South stars. Indeed the discordances of the Melbourne observations are so considerable that Mr. Stone is inclined to look for their origin in some physical cause, and he suggests whether they may not be attributable to the fact of the strata of the atmosphere towards the south, at Melbourne, being more loaded with moisture than the strata towards the north.

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I may add that the result of Mr. Stone's refraction investigation has been adopted for use at the Royal Observatory, Greenwich, with Mr. Airy's approbation.

I have thus, Gentlemen, endeavoured to give a faint outline of this paper of Mr. Stone's. Perhaps the time may not yet have arrived for a thorough re-casting of the great subject to which it refers. But of this I feel assured, that you will all agree with me in recognising the ability and skill which the author has displayed in his researches. The residual discordances which he has so clearly pointed out may not, under existing circumstances, admit of an explanation which is satisfactory in all respects, but the tracing out such anomalies cannot fail to lead to further advances in science.

We owe to Mr. Stone two papers on the Constant of Lunar Nutation which have been recently communicated to the Society.

In the first of these he has investigated the value of the nutation constant deducible from the observations in north polar distance of *Polaris*, *Cephei* 51, *δ Ursæ Minoris*, made with the Greenwich Transit Circle between the years 1851 and 1865 inclusive.

In his second paper he has investigated the value of the same element derivable from the observations in right ascension of *Polaris*, made also with the Greenwich Transit Circle between the years 1851 and 1867.

In the first of these investigations he had a difficulty to contend with arising from the circumstance of different division errors and different values of astronomical flexure having been employed successively in the reduction of the observations throughout the period of fifteen years over which they extended. Furthermore there were used in the process of reduction nadir points determined by a combination of reflecting eye-piece results with those obtained by observations of stars by reflexion.

The first step of the author, therefore, was to investigate the numerical corrections applicable to the registered north polar distances of the three stars, in order that the results might be regarded as equivalent to having been reduced according to one uniform system. Again a still further correction was required to be applied in consequence of the nutation constant used in the reductions during the earlier part of the period having been different from that used in the latter part.

Discussing, then, the observations of each star in succession the author arranges them in yearly groups, and in this manner forms a system of fifteen equations of condition involving three unknown quantities, a correction to the assumed nutation constant, a correction to the assumed north polar distance of the star, and a quantity representing the star's proper motion.

Finally, solving these equations, he obtains a value of the nutation constant affected by a quantity representing the proper motion of the star, which he leaves in the meantime undetermined.

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In his second paper, which is devoted to a discussion of the mean right ascensions of *Polaris*, Mr. Stone similarly collects the observations into yearly groups, and by means of the resulting materials forms a system of seventeen equations of condition involving as unknown quantities the correction to the nutation constant, the correction to the assumed mean right-ascension of the star, and a correction to the proper motion of the star.

Solving, then, the equations by the method of least squares, he obtains the value of the nutation constant, affected with an undetermined correction to the proper motion of the star. Finally, adopting the proper motions of *Polaris* in R.A. and N.P.D., and of *Cephei* 51, and δ *Ursæ Minoris* in N.P.D., as given by Gould in his *Standard Places of Fundamental Stars*, he determines the definitive value of the constant of nutation to be $9''\cdot134$.

This result is slightly smaller than the result of any other value of the same constant which has been derived from the researches of astronomers in the present century, with the exception of the value obtained by Lindenau.

It is to be borne in mind, however, that the observations which form the groundwork of Mr. Stone's researches were all made with the same instrument, and that, too, one of the most powerful in existence for astronomical purposes, that they were carefully cleared of every known source of instrumental error, that they were all reduced according to one uniform system, and were finally subjected to a most thorough discussion.

Under these circumstances I feel bound to consider that the result obtained by Mr. Stone for the value of the nutation constant must be regarded as definitive, in so far as it is derivable from the Greenwich Observations of the present time.

In the former of his two papers on this subject Mr. Stone remarks that the corrections to the co-latitude, as determined from the yearly means of the observations of *Polaris* above and below pole, were affected by irregularities which seemed to indicate the existence of a systematic error proceeding from some undetermined cause. Assuming that these outstanding irregularities depended on the position of the Moon's node, he investigated the effects hence arising, and, upon comparing the results with those assigned by observation, he arrives at the conclusion that there may be really some grounds for supposing that the Greenwich co-latitude is affected by a periodical change. Admitting the existence of such a period he suggests whether it may not be attributable to a yielding of the Earth's crust under the Moon's action, or rather perhaps to a systematic deformation of the Earth's atmosphere arising from the same cause.

I have deemed it proper to bring under your notice this supplement of Mr. Stone's paper because from the admirable skill with which the author has eliminated every known source of error from the co-latitude corrections the outstanding anomalies acquire a value which would not otherwise attach to them.

Anomalous irregularities affecting the co-latitude of the

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Greenwich Observatory, it is well known are not new. The true cause of these irregularities will no doubt be one day thoroughly ascertained. In the meantime any investigation such as that which we owe to Mr. Stone is invaluable as constituting a clue which may lead to their discovery.

It remains for me to glance briefly at one or two of the other papers which Mr. Stone has communicated to the Society.

It is well known that the stars when seen through a good telescope exhibit disks of sensible magnitude, surrounded to some distance beyond by an alternation of dark and bright rings.

The explanation of these rings upon the principles of the undulating theory of light is due to the Astronomer Royal (*Trans. Phil. Soc. Camb.* vol v.).

It may be shown by theory that the radius of any one of those rings varies inversely as the aperture.

In the *Monthly Notices* for December 1865 there is published a short paper by Mr. Stone upon this subject.

In this communication he has given measures from theory of the relative illumination of the disk at twenty different distances from the centres. The results show that there is a rapid degradation of the light of the disk towards the first black ring.

The author accordingly draws the conclusion that it by no means follows from the theory that the angular diameter of the *visible* disk varies inversely as the radius of the aperture.

Mr. Stone has contributed several papers to the *Monthly Notices*, having for their object the elucidation of certain interesting points in Physical Astronomy. I shall here confine myself to a brief allusion to one of these, a paper on the determination of the Moon's mass, which is published in the *Monthly Notices* for Jan. 1868.

In this communication to the Society Mr. Stone has investigated independently the expressions for lunisolar precession and nutation derivable from theory, and assigning to the constants of those elements their most accredited values, he hence determines the Moon's mass to be $\frac{1}{81\cdot36}$.

In a short paper which has been published in the *Monthly Notices*, Mr. Stone has considered the very important question of the circumstances which determine the rejection of discordant observations.

Distinguishing the ordinary class of graduated errors from errors due to accidental causes, he then proceeds to investigate a formula for the rejection of discordant observations, founded upon the assumption of a definite ratio of the mass of the observations being affected with errors due to accidental causes.

No doubt the assumption is to a certain extent arbitrary, but the principle is simple, and leads to a criterion for the rejection of discordant observations the arbitrariness of which is confined within comparatively narrow limits.

I have thus, Gentlemen, endeavoured to submit to you a state-

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ment of the facts which have influenced the Council in their deliberations with reference to the award of the Medal for the present year.

The President then presented the Medal to Mr. Stone, addressing him in the following terms:—

Mr. Stone, in presenting you with this Gold Medal I know that I am not only placing in your hand the most honourable testimony of your great zeal and talent which it is in the power of this Society to bestow, but I feel convinced that every astronomer will recognise the award as a just tribute offered by the Society to the eminent ability, the energy, and assiduity which have distinguished your labours for the advancement of Astronomy.

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The Meeting then proceeded to the election of the Officers and Council for the ensuing year, when the following Fellows were elected:—

President:

Admiral R. H. MANNERS.

Vice-Presidents:

J. C. ADAMS, M.A. F.R.S., Lowndean Professor of Astronomy, Cambridge.
A. CAYLEY, M.A. F.R.S., Sadlerian Professor of Geometry, Cambridge.
WARREN DE LA RUE, Ph. D. F.R.S.
Rev. CHARLES PRITCHARD, M.A. F.R.S.

Treasurer:

SAMUEL CHARLES WHITBREAD, Esq., F.R.S.

Secretaries:

WILLIAM HUGGINS, Esq. F.R.S.
EDWARD J. STONE, Esq. M.A. F.R.S.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIX. *March* 12, 1869. No. 5.

Admiral MANNERS, President, in the Chair.

Col. Addison, 2nd Regiment of Foot;
Chas. Barton, Esq., H.M.S. *Conway*, Rock Ferry, Liverpool;
Rev. R. Prichard Davies, Hatfield, Herts;
Lieut. George Strahan, Uxbridge Villas, Slough;

were balloted for and duly elected Fellows of the Society.

On the Aden and Guntoor Photographs of the Eclipse of the Sun, August 18, 1868. By Warren De La Rue, Esq.

In a communication published in the *Monthly Notices*, vol. xxix., No. 3, I called attention to differences in the representations of the prominence called the "Great Horn," as depicted in a woodcut of the Aden Photographs in the *Engineer* of November 6 and 13, 1868, and in the fac-similes of the drawings made by the several observers along the line of totality, when compared with the Guntoor photographs. I therein stated that these differences in the several aspects of the prominence in question might be accounted for on the supposition that it was undergoing an axial rotation during the period that it was being observed at the several stations.

I pointed out, however, that this hypothesis depended mainly upon the faithfulness of the copy of the Aden photograph, as given in the *Engineer*, and that it would not be possible to speak with certainty on this point until prints of the Aden and Guntoor

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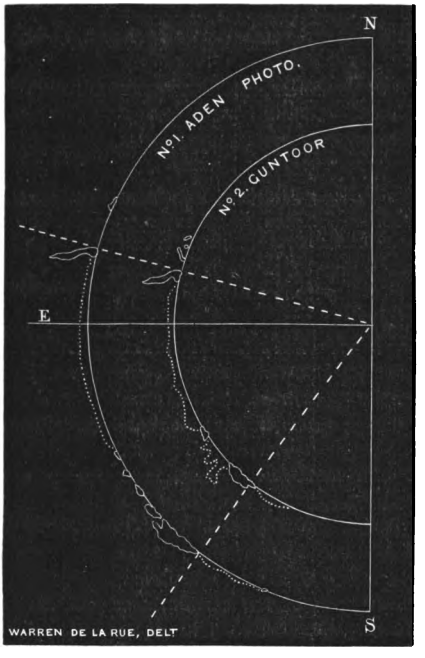
photographs had been compared. I had endeavoured to procure copies of the Aden photographs before I sent in my paper, but was not successful; since its publication, however, I have obtained a copy of No. 1 Aden photograph, through the kindness of Dr. Foerster, Director of the Berlin Observatory.



I have now the honour to lay before the Society a tracing of this photograph, combined with one of No. 2 Guntoor photograph, in such a manner that they may be directly compared.

It will be seen that there is a great difference between the woodcut of the Aden photograph in the *Engineer* and the tracing herein given; and that so far from the Great Horn being so curved that its point is directed northward, the point (as is the case in the Guntoor photographs) is directed southward; moreover, there is not much difference in the aspect of this prominence in these pictures taken 40 minutes apart. The inclination, N. towards E., however, of the point to a radius drawn tangentially to the northern boundary of the base is 6° less in the Aden photograph (24° 20′) than it is in the Guntoor photograph, 30° 20′. A comparison of the two pictures makes it evident, therefore, that some change in the direction of the Great Horn had occurred in the lapse of 40 minutes, but it is not possible to say with certainty whether or not it was due to an axial rotation of this prominence.

On the Transit of Mercury in 1868, *and on* γ^1, γ^2, *and* γ^3 *Andromedæ in Daylight.* By Thomas Barneby, Esq.

Having read accounts of the different appearances presented to different observers of the transit of *Mercury* across the disk of the Sun on the 5th November, 1868, I think an account of another observation of it, with a large refractor, may be interesting and perhaps useful.

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In my locality the sky was cloudless and the air steady, and as soon as I could reach the Sun with my Equatoreal, at about $7^h\ 45^m$ G.M.T., I saw a well-defined very black but dull circular image of *Mercury* projected on the Sun. I watched it, with a friend, from that time until the time of the planet's disappearance at about $9^h\ 2^m\ 41^s$ G.M.T.

I saw no bright spot on the planet, nor any aureola around it, but I several times noticed on it a streak of light from south to north towards the west, in the shape of twisted lightning, which I attributed to ocular delusion.

I perceived no filament thrown out from the planet on approaching last internal contact, nor was there any distortion interfering with circularity, apparent to me of either celestial body during the progress of the transit, although the edge of each seemed slightly serrated.

I may mention, as a test of my telescope and of my sight, that on the 5th of February, 1869, at about $5^h\ 45^m$ G.M.T., with the same instrument, I saw γ^2 and γ^3 *Andromedæ* with a well-defined line of division between them, whilst there was daylight by which I read the R.A. and D. of their primary, and set the circles of my instrument.

γ^1 had a well-defined clean disk of a deep orange colour: γ^2 was a light yellow, and γ^3 distinctly blue. I observed to my companion, "The smaller star separated from the other is blue," without recollecting that it had been seen so before.

My view of the Sun was with a solar diagonal eye-piece, and a power of 113 applied to it, and that of γ *Andromedæ* with a direct eye-piece, and a power of 720, having the whole 9-inches aperture of my object-glass exposed on each occasion.

South Villa Observatory, near Worcester,
8 *February*, 1869.

Observations on the Transit of Mercury, 5th November, 1868, *Hobart Town.* By Thomas Abbott, Esq.

I transmit a diagram with the following observations, made with an equatoreally-mounted achromatic of 4-inches aperture and 5-feet focal length. Other observations were made at the same time and near the same place by an assistant, with a small Gregorian reflector. The day was more than usually fine, and the whole passage of the planet over the Sun well seen. According to the value imparted to such a transit for correcting or verifying the longitude, time being deduced from the *Nautical Almanac*, and when compared with the adopted longitude of the place, this transit, as near as observation could be made, was remarkably close.

The Royal Astronomical Society are in possession of all the means employed by Commander Kay for determining the geo-

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graphical position of the Magnetic Observatory here, the whole series of observations having been reduced for them by Captain Shadwell, which gave for the longitude $9^h\ 49^m\ 29^s{\cdot}6$. Since the time this result was arrived at, Captain Denham, when stationed at Garden Island,* sent into these waters an officer and crew for the purpose of testing the longitude of Hobart Town. The result of all observations made at this time gave for the longitude $9^h\ 49^m\ 28^s{\cdot}8$, being a small difference of $0^s{\cdot}8$ (eight-tenths of a second). There remains, however, much doubt as to to the correct geographical position of any of the Australian colonies being well known. Captain Kay thinks with the Astronomer Royal that for observing the forthcoming transit of *Venus* they are not suited until better known. Mr. Ellery thinks differently, he considers the position of the Melbourne Observatory to be as well known as that of the Cape of Good Hope.

There is very great uncertainty of any expedition being able to reach either Wilks' Land or Sabrina Land in the month of December. If, therefore, some well-defined spot could be fixed upon, and suitable means employed to establish it as a starting-point for determining the correct geographical position of any other spot with equal accuracy, there would be found no difficulty in making correct, for the reception of any expedition that might be sent for the purpose, any position of the Australian Colonies, either by telegraphic communication, transmission of chronometers, or such other means as might be thought most suitable.

The Diagram accompanying the foregoing paper shows—

		h	m	s	
External contact at	Ingress	3	14	9	Hobart Town M.T.
,, ,,	Egress	6	52	27	,,

and least distance of line of transit from centre = 12′ 15″, the Sun's semidiameter being 16′ 11″.

Private Observatory,
Hobart Town, Tasmania,
28 *December*, 1868.

Observations of the Transit of Mercury over the Sun's Disk on Nov. 4th, 1868, at the Royal Observatory, Cape of Good Hope, with Equations deduced therefrom. By W. Mann, First Assistant at the Observatory.

With the 8½-foot Equatoreal, the following observations at ingress and egress were made by Sir T. Maclear and Mr. G. Maclear.

* The established place of observation for all the officers of H.M. ships visiting Sydney.

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Ingress.

	Chron. Times. h m s	Cape M.T. h m s	Obs.	Remarks.
Mercury apparently bisected by the Sun's limb	18 48 16·7	18 40 3·5	T.M.	Good.
Interior contact of limbs ..	18 49 48·0	18 41 34·80	G.M.	Good.

Egress.

Interior contact of limbs ..	22 23 0·0	22 14 46·68	T.M.	
Mercury apparently bisected by the Sun's limb	22 24 16·0	22 16 2·68	T.M.	
Exterior contact of limbs ..	22 25 20·5	22 17 7·18	T.M.	

The sky was partially and thinly clouded at egress, through which the objects were seen with sufficient clearness, but the limbs were badly defined and tremulous.

The powers used were 200 at the ingress, and 440 at the egress.

The following observations were made with a divided object-glass micrometer, attached to the Dollond telescope of 46-in. focal length, and $3\frac{1}{2}$-in. aperture, employing a power of 80 throughout, and they consist of measures of the distances of both limbs of *Mercury* from the nearest and furthest points on the Sun's limb.

The four different classes of observation are distinguished by the letters A, B, C, D, thus :—

A denotes an internal contact of the limb of *Mercury* with the nearest point on the Sun's S. limb.

B denotes an external contact of the limb of *Mercury* with the nearest point on the Sun's S. limb.

C denotes an internal contact of the limb of *Mercury* with the furthest point on the Sun's N. limb.

D denotes an external contact of the limb of *Mercury* with the furthest point on the Sun's N. limb.

The plan of operations it was proposed to follow during the progress of the transit was only partially and imperfectly carried out, owing to the unfavourable state of the sky.

From the commencement to set (6), the sky was clear, and the images of the Sun and planet, although faint, were sharp and steady; but from set (7) to the close, the Sun was repeatedly clouded, and the observations for the most part were obtained with great difficulty, and their accuracy undoubtedly much impaired by the extreme faintness and indistinctness of the images.

With three exceptions, five measures of each contact of the limbs were obtained; and the mean of the measures is considered sensibly to correspond with the mean of the times.

The following table contains all the measures that were taken, with the chronometer times, and the means of each group.

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No. of Set.	Class of Obs.	Chron. Times. h m s	Scale Reading.	Remarks.
1	A	18 51 10	0·081	
		52 12	·090	
		53 34	·104	
		54 45	·116	
		55 28	·123	
		18 53 25·8	0·1028	
2	B	18 57 45	0·162	
		58 21	·168	
		58 56	·174	
		59 40	·178	
		60 16	·184	
		18 58 59·6	0·1732	
3	C	19 4 22	4·433	
		5 10	·430	
		6 2	·422	
		7 5	·414	
		7 35	·409	
		19 6 2·8	4·4126	
4	D	19 8 18	4·424	
		8 57	·420	
		9 42	·414	
		10 13	·411	
		11 16	·402	
		19 9 41·2	4·4142	
5	A	19 15 28	0·273	
		16 7	·280	
		16 45	·282	
		17 17	·286	
		18 10	·291	
		19 16 45·4	0·2824	
6	B	19 19 21	0·320	
		19 58	·324	
		20 43	·333	
		21 19	·338	
		22 2	·344	
		19 20 40·6	0·3318	

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No. of Set.	Class of Obs.	Chron. Times. h m s	Scale Reading.	Remarks.
7	C	19 25 8	4·278	Clouded; both objects very faint.
		25 52	·275	
		26 28	·272	
		27 10	·269	
		28 15	·260	
		19 26 34·6	4·2708	
8	D	19 29 18	4·274	Clouded; both objects very faint.
		30 36	·272	
		32 40	·258	
		33 30	·253	
		34 23	·247	
		19 32 5·4	4·2608	
9	A	19 37 24	0·412	Clouds.
		38 30	·419	
		39 13	·421	
		39 58	·425	
		43 35	·442	
		19 39 44·0	0·4238	
10	B	19 45 12	0·474	The Sun frequently obscured by cloud.
		46 35	·482	
		48 32	·490	
		51 8	·498	
		53 6	·510	
		19 48 54·6	0·4908	
11	D	20 5 3	4·123	In these measures the contact at the S. limb of *Mercury* instead of the N. limb was inadvertently taken.
		5 26	·125	
		6 7	·122	
		20 5 32·0	4·1233	
12	C	20 16 22	4·083	From 20^h to $20^h\ 30^m$ a few glimpses only of the Sun were obtained.
		16 58	·076	
		20 16 40	4·0795	
13	D	20 30 54	4·090	After taking this set the Sun became wholly obscured for about 45^m.
		31 38	·091	
		32 18	·097	
		33 0	·097	
		33 32	·098	
		20 32 16·4	4·0946	

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No. of Set.	Class of Obs.	Chron. Times. h m s	Scale Reading.	Remarks.
14	A	21 36 7	0·310	
		36 44	·306	
		37 25	·300	
		38 12	·298	
		38 40	·292	
		21 37 25·6	0·3012	
15	B	21 39 25	0·311	
		39 55	·307	
		40 22	·308	
		41 8	·300	
		41 55	·295	
		21 40 33·0	0·3042	
16	C	21 46 20	4·388	The Sun clouded; the observations very uncertain.
		48 16	·408	
		49 0	·406	
		51 42	·430	
		52 23	·432	
		21 49 33·2	4·4128	
17	D	21 53 5	4·457	Clouded; the observations very uncertain.
		53 40	·459	
		54 17	·463	
		55 5	·477	
		55 45	·478	
		21 54 22·4	4·4668	
18	A	22 7 44	0·064	
		8 4	·066	
		8 25	·062	
		9 0	·055	
		9 28	·051	
		22 8 32·2	0·0596	
19	B	22 10 0	0·064	Very faint; further observation impracticable.
		10 32	·060	
		22 10 16·0	0·0620	

The above measures were made by myself, the scale being read by Mr. Sinfield.

The chronometer errors, obtained from comparisons with the transit-clock before and after the observations were, at $18^h\ 16^m\ 50^s$,

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chronometer fast $23^s{\cdot}75$, and at $22^h\ 28^m\ 0^s$ fast, $24^s{\cdot}09$ on Cape Mean Solar Time.

The scale value in arc was deduced from a set of measures of the Sun's horizontal diameter at $22^h\ 45^m$; twelve measures gave 4·6540 in., or corrected for index error, 4·6567 for the apparent diameter; and the tabular value being $32'\ 21''{\cdot}56$, we have

$$1 \text{ inch on the Scale} = \frac{1941''{\cdot}56}{4{\cdot}6567} = 416''{\cdot}94.$$

The index error of the scale was derived from contacts of the moveable image of *Mercury* at alternate sides of the fixed image. Six positive and six negative readings gave 0·0027-in. for the correction to be *added* to a scale reading.

The following table contains an abstract of the means of the foregoing groups of observations, the times being corrected for chronometer error, and the scale readings corrected for index error, and converted into arc.

Set.	Class of Measure.	Cape Mean Times. h m s	Measure. ″	No. of Measures.
1	A	18 53 2·0	43·99	5
2	B	58 35·8	73·34	5
3	C	19 5 39·0	1844·66	5
4	D	9 17·4	1841·58	5
5	A	16 21·6	118·87	5
6	B	20 16·8	139·47	5
7	C	26 10·8	1781·79	5
8	D	31 41·5	1777·62	5
9	A	39 20·1	177·82	5
10	B	48 30·7	205·76	5
11	D	20 5 8·1	1720·29	3
12	C	16 16·1	1702·03	2
13	D	31 52·5	1708·32	5
14	A	21 37 1·6	126·71	5
15	B	40 9·0	127·96	5
16	C	49 8·2	1841·00	5
17	D	53 58·4	1863·50	5
18	A	22 8 8·1	25·98	5
19	B	9 51·9	26·98	2

In addition to the foregoing, the times of external and internal contact at ingress were noted as follow:—

	h m s		h m s	
External contact of limbs	18 40 6	by chron. or	18 39 42·2	Cape M.T.
Internal contact ,,	18 41 58	,,	18 41 34·2	,,

The time of external contact is of little value, and the instant

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of internal contact is not very accurate, owing to an indistinctness of limb caused by a slight overlap of the images in a direction at right angles to the line of section of the lenses.

In consequence of this error of adjustment—for the correction of which no means exist in this instrument—measures of the diameter of *Mercury* were not attempted with the divided object-glass.

Reduction of the Observations.

The first step was the calculation of the apparent distances of the centres of the Sun and *Mercury* for the times of ingress and egress observed with the 8½-foot Equatoreal, taking the contacts of the limbs only; also for the times given in the foregoing abstract of measures, and with elements derived from the *Nautical Almanac*.

With Greenwich Mean Solar times corresponding to the above Cape Mean Solar times, the Right Ascensions and South Polar Distances of the Sun and *Mercury* were interpolated with second and third differences.

The parallaxes in Right Ascension and Polar Distance were calculated with the aid of tables adapted to the latitude of the place, and of a similar form to those described in various Introductions to the Greenwich Observations, containing quantities D and ϕ, calculated for every minute of hour-angle, and determined by the formulæ

$$\text{Sin D} = \text{sin geocentric colat.} \times \text{sin hour-angle},$$
$$\tan \phi = \tan \text{geocentric colat.} \times \cos \text{hour-angle},$$

with which we obtain

$$\text{the Parallax in R.A. (in arc)} = \frac{\text{hor. eq. parallax} \times r \times \sin \text{D}}{\sin \text{S.P.D.}}$$
$$\text{the Parallax in P.D.} = \text{hor. eq. parallax} \times r \,.\, \cos \text{D} \,.\, \sin (\text{S.P.D.} - \phi)$$

r being the geocentric radius for the Cape, in terms of the equatoreal radius.

The tabular and the apparent Right Ascensions and South Polar Distances of the Sun and *Mercury* are exhibited in the subjoined table. The numerals I, II, and III, in the first column serve to distinguish the quantities depending on the time of internal contact at ingress and the times of internal and external contact at egress; the other numerals refer to the observations made with the divided object-glass.

In the calculation of the parallaxes in R.A. and S.P.D. the tabular equatoreal horizontal parallax of *Mercury* was taken $= 12''{\cdot}70$, and that of the Sun $= 8''{\cdot}66$.

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No. of Set.	Sun's Tabular R.A.	Par. in R.A.	Sun's Apparent R.A.	Sun's Tabular S.P.D.	Par. in S.P.D.	Sun's Apparent S.P.D.
	° ′ ″	″	° ′ ″	° ′ ″	″	° ′ ″
I	220 39 20·07	7·24	220 39 27·31	74 12 57·85	4·14	74 13 1·99
1	39 48·51	7·14	39 55·65	12 49·18	4·04	12 53·22
2	40 2·36	7·08	40 9·44	12 44·96	4·00	12 48·96
3	40 19·88	7·00	40 26·88	12 39·62	3·94	12 43·56
4	40 28·94	6·96	40 35·90	12 36·86	3·91	12 40·77
5	40 46·52	6·87	40 53·39	12 31·51	3·86	12 35·37
6	40 56·25	6·82	41 3·07	12 28·54	3·83	12 32·37
7	41 10·92	6·74	41 17·66	12 24·07	3·78	12 27·85
8	41 24·62	6·66	41 31·28	12 19·90	3·74	12 23·64
9	41 43·64	6·54	41 50·18	12 14·11	3·68	12 17·79
10	42 6·45	6·39	42 12·84	12 7·16	3·61	12 10·77
11	42 47·78	6·10	42 53·88	11 54·58	3·49	11 58·07
12	43 15·48	5·88	43 21·36	11 46·15	3·42	11 49·57
13	43 54·27	5·55	43 59·82	11 34·35	3·32	11 37·67
14	46 36·12	3·93	46 40·05	10 45·05	2·96	10 48·01
15	46 44·06	3·84	46 47·90	10 42·69	2·94	10 45·63
16	47 6·41	3·58	47 9·99	10 35·89	2·90	10 38·79
17	47 18·44	3·44	47 21·88	10 32·24	2·88	10 35·12
18	47 53·66	3·03	47 56·69	10 21·53	2·83	10 24·36
19	47 57·98	2·98	48 0·96	10 20·22	2·83	10 23·05
II	48 10·20	2·83	48 13·03	10 16·51	2·81	10 19·32
III	220 48 16·02	2·76	220 48 18·78	74 10 14·73	2·80	74 10 17·53

No. of Set.	Mercury's Tabular R.A.	Par. in R.A.	Mercury's Apparent R.A.	Mercury's Tabular S.P.D.	Par. in S.P.D.	Mercury's Apparent S.P.D.
	° ′ ″	″	° ′ ″	° ′ ″	″	° ′ ″
I	220 43 27·54	10·63	220 43 38·17	73 57 23·65	6·05	73 57 29·70
1	42 53·12	10·48	43 3·60	57 44·49	5·91	57 50·40
2	42 36·39	10·40	42 46·79	57 54·62	5·84	58 0·46
3	42 15·20	10·28	42 25·48	58 7·45	5·76	58 13·21
4	42 4·25	10·22	42 14·47	58 14·09	5·71	58 19·80
5	41 43·01	10·09	41 53·10	58 26·95	5·63	58 32·58
6	41 31·22	10·02	41 41·24	58 34·09	5·59	58 39·68
7	41 13·49	9·90	41 23·39	58 44·82	5·52	58 50·34
8	40 56·93	9·78	41 6·71	58 54·85	5·45	59 0·30
9	40 33·95	9·61	40 43·56	59 8·77	5·37	59 14·14
10	40 6·39	9·38	40 15·77	59 25·47	5·27	59 30·74
11	39 16·47	8·94	39 25·41	59 55·72	5·09	74 0 0·81
12	38 43·02	8·62	38 51·64	74 0 15·99	4·98	0 20·97
13	37 56·15	8·14	38 4·29	0 44·40	4·83	0 49·23
14	34 40·58	5·73	34 46·31	2 43·01	4·31	2 47·32
15	34 31·22	5·60	34 36·82	2 48·69	4·29	2 52·98
16	34 4·25	5·22	34 9·47	3 5·06	4·23	3 9·29
17	33 49·76	5·02	33 54·78	3 13·86	4·20	3 18·06

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No. of Set.	Mercury's Tabular R.A.	Par. in R.A.	Mercury's Apparent R.A.	Mercury's Tabular S.P.D.	Par. in S.P.D.	Mercury's Apparent S.P.D.
	° ′ ″	″	° ′ ″	° ′ ″	″	° ′ ″
18	220 33 7·28	4·40	220 33 11·68	74 3 39·64	4·13	74 3 43·77
19	33 2·09	4·32	33 6·41	3 42·79	4·12	3 46·91
II	32 47·35	4·11	32 51·46	3 51·74	4·09	3 55·83
III	220 32 40·33	4·00	220 32 44·33	3 56·00	4·08	74 4 0·08

If now we put

a for the excess of the apparent R.A. of the Sun's centre above that of *Mercury's* centre;

p for the excess of the apparent S.P.D. of the Sun's centre above that of *Mercury's* centre;

Δ and δ for the S.P.D.'s of the Sun and *Mercury*;

then, the apparent distance of the centres $(c) = p \sec \zeta$,

$$\text{where } \tan \zeta = \frac{a}{p} \sqrt{\sin \Delta \sin \delta}.$$

Also, let σ and μ be the semidiameters of the Sun and *Mercury*, and m' the tabular value of the measured distance of the limbs; then for the several classes of observations with the divided object-glass micrometer, we shall have

For Class A, $m' = \sigma - \mu - c$, Class C, $m' = \sigma - \mu + c$,

B, $m' = \sigma + \mu - c$, D, $m' = \sigma + \mu + c$,

At the instant of ingress or egress, $m' = 0$

and we have

For the internal contact, $c = \sigma - \mu$; for the external contact, $c = \sigma + \mu$.

Substituting the tabular values of σ and μ, viz. $\sigma = 970''{\cdot}78$ and $\mu = 4''{\cdot}94$, we have

For A, $m' = 965''{\cdot}84 - c$ for C, $m' = 965''{\cdot}84 + c$

B, $m' = 975''{\cdot}72 - c$ D, $m' = 975''{\cdot}72 + c$

The values of a, p, c, and m', are given below:—

No. of Set.	Class of Observation.	a.	p.	c.	m'.
		′ ″	′ ″	″	″
I	A	− 4 10·86	15 32·29	963·00	0·00
1	A	3 7·95	15 2·82	920·74	45·10
2	B	2 37·35	14 48·50	901·29	74·43
3	C	1 58·60	14 30·35	877·79	1843·63
4	D	1 38·57	14 20·97	866·17	1841·89
5	A	0 59·71	14 2·79	844·74	121·10
6	B	0 38·17	13 52·69	833·50	142·22
7	C	− 0 5·73	13 37·51	817·53	1783·37

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No. of Set.	Class of Observation.	*a*.	*p*.	*c*.	*m*.
		′ ″	′ ″	″	″
8	D	+ 0 24·57	13 23·34	803·69	1779·41
9	A	1 6·62	13 3·65	786·26	179·58
10	B	1 57·07	12 40·03	768·32	207·40
11	D	3 28·47	11 57·26	744·75	1720·47
12	C	4 29·72	11 28·60	735·84	1701·68
13	D	5 55·53	10 48·44	733·07	1708·79
14	A	11 53·74	8 0·69	838·04	127·80
15	B	12 11·08	7 52·65	847·25	128·47
16	C	13 0·52	7 29·50	875·00	1840·84
17	D	13 27·10	7 17·06	890·86	1866·58
18	A	14 45·01	6 40·59	940·77	25·07
19	B	14 54·55	6 36·14	947·22	28·50
II	A	15 21·57	6 23·49	965·79	0·00
III	B	+15 34·45	6 17·45	974·82	0·00

Correction of the Measured Distances of the Limbs for the Effects of Refraction.

Calling the vertical refraction $= \alpha \tan z$, where z is the apparent zenith distance; also putting γ for the inclination of the measured distance m, to the vertical circle passing through its middle point; then the effect of the refraction on the measured distance will be given by the formula

$$\alpha m \sec^2 \psi \sin 1'', \text{ where } \tan \psi = \cos \gamma \tan z.$$

It will be sufficiently accurate for all the cases considered in the present discussion to calculate z and γ for the centre of the Sun at the instant of observation.

With the Sun's hour-angle and S.P.D, and with the aid of tables calculated for the latitude of the Cape, having arguments to every minute of hour-angle, the values of z_0 the true zenith distance, and of S. his angle of situation were readily obtained. The true zenith distances were easily converted into apparent Z.D.'s with the aid of a table of refractions; and the values of γ were found with all requisite accuracy from the expression $\gamma = S - \zeta$, where ζ, which is already calculated, is the inclination of the distance of the centres c to the circle of declination passing through its middle point.

The principal numerical quantities involved in the calculation of the corrections for refraction, together with the measures corrected and compared with the tabular values are given below.

The following are the readings of the barometer and thermometer used in calculating the values of log α from the table of refraction:—

Set 1 to 10	Set 11 to 13	Set 14 to 19
Bar. 30^{in}·19	Bar. 30^{in}·20	Bar. 30^{in}·22
Ther. { Att. 58°·0 / Out 54°·0	Ther. { Att. 59° / Out 56°	Ther. { Att. 60° / Out 58°·5

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Set.	Sun's Hour Angle. h m s	Apparent Z.D. of Sun's Centre (z). ° ′	Angle of Situation (s). ° ′	ξ. ° ′	γ. ° ′
1	4 50 42	67 2	59 19	−11 19	70 38
2	4 45 8	65 53	59 24	9 40	69 4
3	4 38 5	64 25	59 29	7 28	66 57
4	4 34 26	63 40	59 31	6 17	65 48
5	4 27 32	62 14	59 33	3 54	63 27
6	4 23 27	61 23	59 34	2 31	62 5
7	4 17 33	60 10	59 34	− 0 23	59 57
8	4 12 2	59 2	59 35	+ 1 41	57 52
9	4 4 24	57 27	59 29	4 40	54 49
10	3 55 13	55 32	59 22	8 26	50 56
11	3 38 36	52 7	59 1	15 37	43 24
12	3 27 28	49 49	58 39	20 39	38 0
13	3 11 51	46 37	57 57	27 48	30 9
14	2 6 42	33 44	51 39	55 0	3 21
15	2 3 35	33 9	51 10	56 6	4 56
16	1 54 36	31 29	49 38	59 5	9 27
17	1 49 45	30 36	48 40	60 37	11 57
18	1 35 36	28 6	45 31	64 48	19 17
19	1 33 52	27 48	45 5	+65 17	20 12

Set.	Log *a*.	Corr. for Refr. ″	Measured Distance. ″	Meas. Dist. Corr. for Refr. (*m*). ″	Calculated Distance. (*m*′). ″	*m* − *m*′. ″
1	1·7619	0·02	43·99	44·01	45·10	−1·09
2	1·7622	0·03	73·34	73·37	74·43	−1·06
3	1·7624	0·86	1844·66	1845·52	1843·63	+1·89
4	1·7626	0·87	1841·58	1842·45	1841·89	+0·56
5	1·7628	0·06	118·87	118·93	121·10	−2·17
6	1·7630	0·07	139·47	139·54	142·22	−2·68
7	1·7631	0·88	1781·79	1782·67	1783·37	−0·70
8	1·7633	0·89	1777·62	1778·51	1779·41	−0·90
9	1·7634	0·09	177·82	177·91	179·58	−1·67
10	1·7636	0·11	205·76	205·87	207·40	−1·53
11	1·7623	0·90	1720·29	1721·19	1720·47	+0·72
12	1·7624	0·89	1702·03	1702·92	1701·68	+1·24
13	1·7626	0·88	1708·32	1709·20	1708·79	+0·41
14	1·7611	0·05	126·71	126·76	127·80	−1·04
15	1·7611	0·05	127·96	128·01	128·47	−0·46
16	1·7611	0·70	1841·00	1841·70	1840·84	+0·86
17	1·7611	0·70	1863·50	1864·20	1866·58	−2·38
18	1·7612	0·01	25·98	25·99	25·07	+0·92
19	1·7612	0·01	26·98	26·99	28·50	−1·51

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Formation of Equations of Condition involving Corrections to the Elements of Computation.

Let

x be the correction to the tabular excess of the Sun's R.A. above *Mercury's* R.A. (in arc);

y be the correction to the tabular excess of the Sun's S.P.D. above *Mercury's* S.P.D.

$d\pi$ the correction to the excess of the parallax of *Mercury* above that of the Sun;

$d\sigma$ and $d\mu$ the corrections to the semidiameters of the Sun and *Mercury*.

Also, let

τ seconds be the correction to the assumed longitude of the Observatory,

that is to say, let the true longitude be

$$-1^h\,13^m\,55^s + \tau \text{ sec.}$$

then, since the relative motions in R.A. and S.P.D. in 1^s of time are

$$+0''{\cdot}0915 \text{ and } -0''{\cdot}0429 \text{ respectively,}$$

we shall have the corrections to a and p, or da and dp, sensibly, as follows:—

$$da = x + {\cdot}0915\,\tau - \varkappa\, d\pi$$
$$dp = y - {\cdot}0429\,\tau - \varkappa'\, d\pi$$

$\varkappa$ and $\varkappa'$ being the coefficients of parallax in R.A. and S.P.D.

Now, since the corrections are all small, we have from the expression $C^2 = a^2 \sin\Delta \sin\delta + p^2$, considering C, a, and p only to vary,

$$dc = \frac{a}{c} \sin\Delta \sin\delta \,.\, da + \frac{p}{c}\, dp,$$

and the final equations for the observed times of ingress and egress will be—

For I. and II. $\sigma - \mu - c = dc - d\sigma + d\mu$; for III. $\sigma + \mu - c = dc - d\sigma - d\mu$.

Also,

$$\text{putting } m = m' + dm',$$

m being the measured and m' the computed distance of the limbs, the final equations for the observations made with the divided object-glass micrometer will be

For class A, $dm' = d\sigma - d\mu - dc$ For class C, $dm' = d\sigma - d\mu + dc$

B, $dm' = d\sigma + d\mu - dc$ D, $dm' = d\sigma - d\mu + dc$

But since a correction $d\sigma$ to the Sun's semidiameter will cause

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a change in the arc value of the measured quantity m, $d\sigma$ in the four last equations must be multiplied by a factor of the form $(1-q)$, where $q = 2\frac{m}{m_0}$, m_0 being the scale measure of the Sun's diameter.

Introducing the numerical value of da, dp, and c, and reducing, we obtain finally—

No. of Equation.							
I.	$-2''84 =$	$+1''000\,d\sigma$	$-1''000\,d\mu$	$+0''241x$	$-''968\,y$	$+''064\,\tau$	$+''260\,d\Pi$
1	$-1''09 =$	$+0''955$	$-1''000$	$+0''189$	$-''981$	$+''059$	$+''301$
2	$-1''06 =$	$+0''925$	$+1''000$	$+0''162$	$-''986$	$+''057$	$+''322$
3	$-1''89 =$	$+0''900$	$+1''000$	$+0''125$	$-''992$	$+''054$	$+''349$
4	$-0''56 =$	$+0''897$	$-1''000$	$+0''105$	$-''994$	$+''052$	$+''363$
5	$-2''17 =$	$+0''878$	$-1''000$	$+0''065$	$-''998$	$+''049$	$+''391$
6	$-2''68 =$	$+0''856$	$+1''000$	$+0''042$	$-''999$	$+''047$	$+''407$
7	$+0''70 =$	$+0''835$	$+1''000$	$+0''007$	$-1''000$	$+''044$	$+''430$
8	$+0''90 =$	$+0''831$	$-1''000$	$-0''028$	$-1''000$	$+''040$	$+''452$
9	$-1''67 =$	$+0''817$	$-1''000$	$-0''078$	$-''997$	$+''036$	$+''482$
10	$-1''53 =$	$+0''788$	$+1''000$	$-0''141$	$-''989$	$+''030$	$+''515$
11	$-0''72 =$	$+0''773$	$-1''000$	$-0''259$	$-''963$	$+''818$	$+''569$
12	$-1''24 =$	$+0''754$	$+1''000$	$-0''339$	$-''936$	$+''009$	$+''597$
13	$-0''41 =$	$+0''760$	$-1''000$	$-0''449$	$-''885$	$-''003$	$+''625$
14	$-1''04 =$	$+0''870$	$-1''000$	$-0''788$	$-''574$	$-''048$	$+''551$
15	$-0''46 =$	$+0''868$	$+1''000$	$-0''798$	$-''558$	$-''049$	$+''541$
16	$-0''86 =$	$+0''896$	$+1''000$	$-0''825$	$-''514$	$-''054$	$+''512$
17	$+2''38 =$	$+0''920$	$-1''000$	$-0''838$	$-''491$	$-''056$	$+''495$
18	$+0''92 =$	$+0''973$	$-1''000$	$-0''870$	$-''426$	$-''061$	$+''441$
19	$-1''51 =$	$+0''972$	$+1''000$	$-0''874$	$-''418$	$-''062$	$+''434$
II.	$-0''05 =$	$+1''000$	$-1''000$	$-0''883$	$-''397$	$-''064$	$+''415$
III.	$-0''90 =$	$+1''000$	$+1''000$	$-0''887$	$-''387$	$-''065$	$+''405$

These equations alone are obviously unfitted for the determination of all the unknown quantities; and, moreover, considering the circumstances under which many of the observations were taken, we should not be justified in assigning an equal weight to them throughout. Values of the weights might be inferred from a consideration of the individual observations, but I have not prosecuted the inquiry further, under the belief that, when the objects were seen faintly through cloud, *constant* errors in the measured distances were as much to be feared as *accidental* errors of observation.

Approximate values of $d\mu$, x and y may be obtained as follows:—

If we take as one equation the mean of Nos. 11 and 13 (No. 11 having been observed by mistake at the S. limb of *Mercury*), and if we exclude I., the remaining twenty equations will give a

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series depending upon equally-balanced observations at both limbs of the planet.

Changing the signs of the equations containing negative values of $d\mu$, and adding, we get

$$20\,d\mu = -8''{\cdot}485 + {\cdot}114\,d\sigma + {\cdot}148\,x - {\cdot}003\,y + {\cdot}003\,\tau - {\cdot}024\,d\pi,$$

or sensibly,

$$d\mu = -0''{\cdot}424.$$

Again, adding together equations 1 to 10, and equations II. to III., considering as before, the mean of 11 and 13 to form one equation, $d\mu$ is eliminated, and we have—

$$-11''{\cdot}05 = 8{\cdot}682\,d\sigma + 0{\cdot}448\,x - 9{\cdot}936\,y + 0{\cdot}468\,\tau + 4{\cdot}012\,d\pi,$$
$$-3''{\cdot}225 = 9{\cdot}020\,d\sigma - 7{\cdot}456\,x - 5{\cdot}625\,y - 0{\cdot}443\,\tau + 4{\cdot}998\,d\pi,$$

whence

$$x = -0''{\cdot}380 + 0{\cdot}532\,d\sigma - 0{\cdot}092\,\tau + 0{\cdot}352\,d\pi,$$
$$y = +1''{\cdot}095 + 0{\cdot}898\,d\sigma + 0{\cdot}043\,\tau + 0{\cdot}420\,d\pi.$$

Assuming the parallaxes to be increased by $\frac{1}{30}$th part, we have $d\pi = +0''{\cdot}13$; and taking $d\sigma = -1''{\cdot}1$, the correction given by the Greenwich Observations of the solar eclipse of 1860, we obtain finally, bearing in mind that y is the correction to the excess of S.P.D.—

Correction to excess of Sun's R.A. over *Mercury's* R A. $= -0''{\cdot}92 - {\cdot}092\,\tau$,
Correction to excess of Sun's N.P.D. over *Mercury's* N.P.D. $= -0''{\cdot}16 - {\cdot}043\,\tau$.

Sir T. Maclear, Director of the Observatory, writes :—

I annex the following, derived from my observations with the 8½-foot Equatoreal, to Mr. Mann's elaborate discussion of his observations with the other instruments.

My wish to obtain a good determination of the diameter of *Mercury* was in a great measure frustrated by frequent interruptions from clouds and indifferent definitions after the first hour of the transit.

Under the powers 200 and 440 the planet presented the image of a black round disk, and (when the definition was good) surrounded by a very faint appearance of light. G. W. H. Maclear observed the first internal contact. I looked immediately after and noted three or four minute black dots in the narrow space between the limbs. I fancied something similar immediately before the last internal contact, but the limbs then were in violent apparent motion. I used a blue-glass eye-screen fixed to a diagonal eye-piece.

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The micrometer of the $8\frac{1}{2}$-foot Equatoreal has only one micrometer screw. There is a zero wire parallel to the micrometer wire, which can be placed at any part of the field. Therefore the telescope must be moved to supply the place of a second micrometer, by trial and error tapping with the finger.

The measures for diameter were in polar distance and by "cross" observation to eliminate the coincidence of the wires. Thus each recorded measure embraced twice the diameter.

One revolution of the micrometer-screw = 26″·069, and the sum of the thickness of the wires = 1″·737.

Each of the following results is the mean of five partial measures of the double diameter:—

Cape Mean Time. h m					
18 59	...	...	...	0″·5662	
19 12	...	...	...	0·5708	
19 22	...	...	...	0·5970	
21 47	...	...	...	0·5818	
21 59	...	...	...	0·5636	
	Mean	...	...	= 0·5759	= 15″·013
					7·507
		Half thickness of wires			·869
		Observed diameter			8·376 Probable error ±0·11.

This result is less than the tabular diameter by 1″·50.

On the Observations of the Transit of Venus in 1874.

By G. B. Airy, Esq., Astronomer Royal.

In the *Compte Rendu* of the French Academy for 1869, February 8, M. V. Puiseux has courteously adverted to my late paper on the Transits of *Venus*, and has suggested that the Transit of 1874 can be advantageously observed by the method of comparison of the intervals between ingress and egress, the ingress and egress being both observed at each of the places adopted for comparison. And he has pointed out stations which give a difference of such intervals a very little larger than the intervals which I have adopted, and intimates that the method suggested by him is perhaps, therefore, superior to mine.

It will be seen in the *Monthly Notices* for 1857, May 8, page 213, second paragraph, and pages 214 and 215, that I had fully considered the method which M. Puiseux has now suggested, and that I had actually named the principal stations which M. Puiseux has now indicated. The matter having thus been fully present to my mind, I can at once explain my reasons for preferring the course which I have lately placed before the Society.

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In M. Puiseux's plan the available time-determination will depend on the aggregate (not the mean) of four observations of ingress and egress. In my plan the available time-determination will depend on the aggregate of two observations of ingress or egress and two determinations of geographical longitude. And the practical question is, Which of these methods is subject to the smaller probable error?

Now I hope that, with reasonable care, the probable error of the geographical longitude will not be more than one-half of the probable error of ingress or egress. Moreover, the geographical position remains, and observations can be repeated in future years for the correction of its longitude; but nothing can repair error in the observation of ingress or egress. These points being considered, in combination with the circumstance that the effects of parallax obtained are sensibly equal, I cannot hesitate in maintaining the opinion that my method is the more accurate.

But I trust that the plan proposed by M. Puiseux will still be carried out. Every series of observations which can really be brought to bear upon this important determination will be valuable.

Royal Observatory, Greenwich,
1869, *February* 23.

Note on the Transit of Venus in 1874; *and an exact Determination of those Points on the Earth's Surface at which internal contacts are most accelerated and retarded by Parallax. With an Addendum referring to the possibility of determining the Solar Parallax by the same sort of Observations in* 1874 *as were made in* 1769. By Richard A. Proctor, B.A.

Having applied geometrical tests to the elements discussed in the paper and maps by the Astronomer Royal (*Monthly Notices*, December 11, 1868), I was led to suspect that some of the corrections which would come to be applied in an exact calculation of the circumstances of the two Transits of *Venus* would be larger than a first view of the subject might suggest I now present some of the results to which my examination of the Transit of 1874 has led me, reserving for a future occasion the examination of the Transit of 1882.

I premise that internal contacts are the phenomena to be specially considered; so that, although I have calculated the position angles for external contacts and passages of *Venus's* centre, I have not thought it necessary to determine the exact points on the Earth's surface at which these phenomena are most affected by parallax. I must also add that I have taken as the basis of my calculation the following elements calculated by Mr. Hind, and obligingly furnished me by the Astronomer Royal.

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Transit of *Venus*, December 8th, 1874.

For the centre of the Earth.

Ingress.

	h	m	s		
External contact	13	46	56	Greenwich Mean Solar Time.	
Internal contact	14	15	57	,,	,,

Egress.

Internal contact	17	57	5	,,	,,
External contact	18	26	5	,,	,,

The elements sent me also contained the position-angles for external contacts, but as these were only given to tenths of degrees, I preferred to recalculate them.

From an examination of these elements I learn that the estimated diameter of *Venus* is about 7364 miles, corresponding to an apparent semidiameter of $8''{\cdot}305$ at the Earth's mean distance, and an assumed equatorial horizontal solar parallax of $8''{\cdot}94$.

The Sun's longitude on December 8, 1874, at 16^h, I estimate at $256^\circ\ 57'$ (with sufficient approximation); the apparent inclination of the Sun's ecliptical diameter to a declination-parallel $5^\circ\ 36'$ (the western end raised towards the north); the Sun's declination $22^\circ\ 49'$ S.

The formulæ I have used are founded on the following considerations. Conceive the Sun and Earth enclosed in a cone whose vertex is beyond the Earth, and also in a double cone whose vertex lies between the Earth and the Sun. These cones will have a common axis,—namely, the Earth's radius vector. Now, it is obvious that when *Venus* has reached the surface of the outer cone, a transit commences at a certain point on the Earth's surface. As soon as *Venus* has reached the surface of the inner cone, transit has begun for all places on the Earth's illuminated hemisphere. The former contact corresponds to the case of ingress accelerated by parallax, the latter to the case of ingress retarded by parallax. Similar considerations apply to egress as affected by parallax.

It is clear that if we suppose the Earth's radius-vector reduced to rest and the motion of *Venus* correspondingly retarded, the remaining motion of *Venus* is that with which she may be assumed to traverse the section of the pair of cones above described. Thus it is easy to calculate the duration of a central passage for external and internal contacts, or for the centre of *Venus*, and either for the outer or inner cone, or for a cone between the two having the Earth's centre for vertex. The case of external contacts for the last-mentioned cone gives the duration of a central transit as seen from the Earth's centre; and by comparing this duration with that given in Mr. Hind's

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elements, we can tell how far the transit of 1874 differs from a central one.

On such considerations the following formulæ and calculations are founded :—

Let D = Sun's diameter.
d = Earth's mean distance.
d' = *Venus's* mean distance.
r = Earth's actual distance.
r' = *Venus's* actual distance.
v = Earth's mean velocity.
v' = *Venus's* mean velocity.
Δ = Earth's mean diameter.
Δ' = *Venus's* mean diameter.
i = inclination of *Venus's* orbit.

Then $v' = v\sqrt{\frac{d}{d'}}$; the actual velocity of the Earth is $v\frac{d}{r}$ (very nearly); that of *Venus* is $v\sqrt{\frac{d}{d'}}\cdot\frac{d'}{r'} = v\frac{\sqrt{d\,d'}}{r'}$; velocity of the Earth's radius vector at *Venus's* distance is $v\frac{d\,r'}{r^2}$. Whence it readily follows that the actual velocity with which *Venus* crosses the section of the cones described above is

$$v = \sqrt{\left\{\frac{\sqrt{d\,d'}}{r'} - \frac{d\,r'}{r^2}\cos i\right\}^2 + \left\{\frac{d\,r'}{r^2}\sin i\right\}^2}$$

$$v = \sqrt{(d^2 r'^4 + d\,d' r'^4 - 2\,d^{\frac{3}{2}} d'^{\frac{1}{2}} r^2 r'^2 \cos i)} + r^2 r'$$

$$= \nabla, \text{ suppose.}$$

Also the cross-section of the cone whose vertex is at the Earth's centre is $D\left(\frac{r-r'}{r}\right)$. Therefore, the duration of a central transit of *Venus's* centre

$$= \frac{1}{\nabla}\left\{\frac{D\,(r-r')}{r}\right\};$$

the duration of a central transit for external contacts

$$= \frac{1}{\nabla}\left\{\frac{D\,(r-r')}{r} + \Delta'\right\};$$

the duration of a central transit for internal contacts

$$= \frac{1}{\nabla}\left\{\frac{D\,(r-r')}{r} - \Delta'\right\};$$

all these phenomena being supposed to be seen from the Earth's

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centre. To determine the interval between phenomena affected by parallax, we have, the section of the outer cone mentioned above

$$= \frac{D\,(r-r')}{r} + \frac{\Delta\, r'}{r};$$

the section of the inner cone mentioned above

$$= \frac{D\,(r-r')}{r} - \frac{\Delta\, r'}{r};$$

and it is readily seen that, for a central transit, the interval between first external contact most accelerated by parallax and last external contact most retarded, is

$$= \frac{1}{V}\left\{\frac{D\,(r-r')}{r'} + \frac{\Delta\, r'}{r} + \Delta'\right\};$$

the interval between first external contact most retarded by parallax and last external contact most accelerated, is

$$= \frac{1}{V}\left\{\frac{D\,(r-r')}{r} - \frac{\Delta\, r'}{r} + \Delta'\right\};$$

the interval between first internal contact most accelerated by parallax and last internal contact most retarded, is

$$= \frac{1}{V}\left\{\frac{D\,(r-r')}{r} + \frac{\Delta' r'}{r} - \Delta'\right\};$$

and, lastly, the interval between first internal contact most retarded by parallax and last internal contact most accelerated, is

$$= \frac{1}{V}\left\{\frac{D\,(r-r')}{r} - \frac{\Delta\, r'}{r} - \Delta'\right\}.$$

By taking the recognised values of D, Δ, Δ', v, d, d', and i; $r = \cdot 98480$ (the Earth's mean distance being unity), and $r' = \cdot 72040$, all the above intervals are readily calculated. The duration of a central transit of *Venus's* centre, seen from the Earth's centre, is found to be $7^h\ 55^m\ 8{\cdot}256^s$. A similar transit calculated for external contacts has a duration of $8^h\ 10^m\ 25^s{\cdot}140$, or $8{\cdot}17365$ hours. Since the duration of the transit of 1874, estimated for external contacts is $4{\cdot}6525$ hours, we have for determining the arc (2θ) of which the actual path of *Venus* is the chord,* the equation

* The arc and chord here referred to belong to a circle having a diameter of $\left\{D\,\frac{(r-r')}{r} + \Delta'\right\}$; but the position-angles eventually deduced may, of course, be referred to the Sun's disk.

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$$\sin \theta = \frac{4{\cdot}6525}{8{\cdot}1736} = \sin 34^\circ\ 41'\ 40''\ \text{approximately};$$

and it is clear the distance separating the chord of transit from the centre of the section is

$$= \frac{1}{2}\left\{\frac{D\,(r-r')}{r} + \Delta'\right\} \cos \theta.$$

This distance is a constant in the calculation, and it will be convenient to call it k.

Now, if we want to determine the duration (t) of the transit of *Venus* across a section of any other radius, as s, and the corresponding arc of transit ($2\,\varphi$) we have the formulæ

$$\cos \varphi = \frac{k}{s};\ \text{and}\ t = \frac{2\,s \sin \varphi}{V}$$

We must give to $2\,s$, successively, the four values included in the expression

$$\left\{\frac{D\,(r-r')}{r} \pm \Delta\,\frac{r'}{r} \pm \Delta'\right\}.$$

In this way the durations were calculated which correspond to the epochs given in the accompanying table. For deducing position-angles from the arcs of transit, we have

The apparent angle at which *Venus* is ascending from the ecliptic towards the west

$$= i + \tan^{-1}\left\{\frac{d\,r'^2 \sin i}{\sqrt{d\,d'}\,r^2 - d\,r'^2 \cos i}\right\} = 9^\circ\ 9' +$$

The angle at which the elliptical diameter of the Sun is inclined to a declination-parallel (west end raised towards the north)

$$= 5^\circ\ 36'.$$

Therefore the apparent angle at which *Venus* is crossing a declination-parallel, rising towards the west, $= 14^\circ\ 45'$.

Thence it readily follows that the arc of transit $2\ (34^\circ\ 41'\ 40'')$ corresponds to the following position-angles,—

External contact at ingress, 130° 33′ from S. through E. towards N.
" " egress, 160° 4′ " " W. "

These values correspond satisfactorily with those given by Mr. Hind, viz. 130°·6 and 160°·0. The same angle 14° 45′ applied to the transit-arcs obtained in the other cases examined, gives the position-angles in the second column of the following table:—

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Elements of the Transit of Venus, December 8th, 1874.

	G. M.T. h m s	Position angle. ° ′
First external contact, most accel. by par.	13 37 35	128 48 from S. to E.
,, ,, from Earth's centre	13 46 56	130 33 ,,
,, ,, most retarded by par.	13 57 47	132 41 ,,
First internal contact, most accel. by par.*	14 3 59	133 56 ,,
,, ,, from Earth's centre	14 15 57	136 28 ,,
,, ,, most retarded by par.*	14 29 5	139 26 ,,
Last internal contact, most accel. by par.*	17 43 58	168 57 from S. to W.
,, ,, from Earth's centre	17 57 5	165 59 ,,
,, ,, most retarded by par.*	18 9 4	163 27 ,,
Last external contact, most accel. by par.	18 15 15	162 12 ,,
,, ,, from Earth's centre	18 26 5	160 4 ,,
,, ,, most retarded by par.	18 35 26	158 19 ,,

To which may be added,

First passage of ♀'s centre across Sun's limb	14 0 51	133 17 from S. to E.
Last ,, ,, ,,	18 12 11	162 48 from S. to W.

(both as seen from the Earth's centre).

The phenomena marked * are those which now concern us. We notice, first, that the maximum interval of time to be determined by the observations applied to the ingress and egress (internal contacts) is $25^m\ 6^s$. This is the interval which would result if each observation were made when the Sun is only raised above the horizon through the effects of refraction. A somewhat longer interval might be dealt with if the effects of refraction were considered, and if the observation of contact were possible when the Sun is close to the horizon. But we need not inquire into the possible extent by which the interval might thus be increased, since observations when the Sun is on the horizon are not available in the present instance. Practically we may consider that an interval of about 20^m has to be dealt with, or at the outside 22^m. As this interval is directly proportional to the solar parallax we can estimate the effect of errors of observation, or of error in the determination of the longitude of a station. An error of one second in the determination of an interval of 21^m would correspond to an error of about $0''{\cdot}007$ in the determination of the solar parallax.

(In an addendum to this Note, I propose to deal with the method of determining the solar parallax by difference of durations, so far as this method is applicable to the transit of 1874. Unless I mistake, I shall be able to show that more is to be hoped from the latter method, than from the one which is the object of the present investigation.)

To return to the consideration of the actual circumstances under which ingress and egress are most affected by parallax.

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To determine the longitude and latitude of the four points on the Earth's surface at which the acceleration and retardation have their maximum values, we have the following formulæ—

Let P = the position-angle, measured from the south point.

δ = Sun's declination.

e = equation of time (additive to mean time).

h = hour at which a phenomenon (ingress or egress) occurs.

l = latitude of point required.

λ' = longitude of point required (l' being estimated from the meridian along which it is apparent noon at the epoch of the phenomenon).

Then $\sin l = \cos P \cos \delta$; and $\cot \lambda' = \cot P \sin \delta$.

I give the formulæ in this form as being that I have made use of. Strictly speaking, a reference should be made to the signs of l, λ'; but the circumstances of each case are quite sufficient to clear up any doubts which might arise. It is obvious that for the first and fourth phenomena marked * in the above table, the latitude is north, and for the others south; because, referring to the preliminary considerations it is seen that the tangent-lines corresponding to the former pair of phenomena belong to a cone whose vertex is outside the Earth, whereas the tangent-lines corresponding to the other phenomena belong to a cone whose vertex lies between the Earth and the Sun. From similar considerations it follows that the longitudes in the first and third cases are eastward of the meridian along which it is noon at the respective epochs, whereas, in the second and fourth cases, the longitudes are westward of the corresponding meridian. With this relation in view, the actual latitude (λ) east of Greenwich is given by the expression

$$\lambda = 24^h - (h + e) \pm \lambda'$$

(*Note.* e changes by a few seconds during the progress of the transit.)

Thus we obtain the following results:—

	Lat.	Long.
(i) The place at which first internal contact is most accelerated lies in	39° 45′ N.	143° 23′ W.
(ii) The place at which first internal contact is most retarded lies in	54 27 S.	29 1 E.
(iii) The place at which last internal contact is most accelerated lies in	64 47 S.	114 22 W.
(iv) The place at which last internal contact is most retarded lies in	62 5 N.	48 30 E.

On a comparison of these positions with those marked in Mr. Airy's maps I., II., III., IV., it will be noticed that the agreement is much closer for the places which fall in north latitude (that is,

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cases I. and IV.) than for the other two. Indeed, as respects the two former positions there is little to affect the general conclusions which the Astronomer Royal has deduced from geometrical considerations. In the case illustrated by Plate I. the point marked with a small circle should be removed some 280 miles towards the north-west, rather westerly, and in the case illustrated by Plate IV. the corresponding point should be removed some 220 miles towards the north-east.

It is in the cases illustrated by Plates II. and III. that the most important changes have to be made. In the first the point marked with a circle should be removed to a distance of upwards of 800 miles towards the south-west. This change is unfavourable so far as the choice of stations for observing the retarded egress is concerned. Fort Dauphin in the Island of Madagascar (where, however, the Sun would be low at the time of internal contact at ingress) and Prince Edward and Marion Islands, are the places which, with Crozet's Island and Kerguelen's Land, are best placed for this observation.

In the case illustrated by Plate III. the change which should be made is not quite so large. The point marked with a circle should be shifted nearly 700 miles towards the south-east, rather easterly. This change is also unfavourable, as it removes the point further away from all the places mentioned by the Astronomer Royal. It follows that Auckland Island, Emerald Island, and Victoria Land, are the places best situated for observing the egress accelerated by parallax.

A portion of the corrections which have here been mentioned are due to the choice of internal contacts as the phenomena to be chiefly attended to. I believe no doubt can exist that this choice is a just and proper one. Indeed, so far as I am aware, no calculations for the determination of solar parallax have ever been founded on attempts to determine the epoch of external contacts.

In a subsequent paper I propose to present the results of calculations similar to the above, applied to the transit of 1882. I believe, however, that the corrections which have to be made in the case of that transit are less considerable than those dealt with above.

Addendum on the possibility of Determining the Solar Parallax by observations applied to the Transit of 1874 *of the same sort as those applied to the Transit of* 1769.

It occurred to me, while considering the relations presented in the above paper that there might possibly be some little inaccuracy in the Astronomer Royal's view that the transit of 1874 is altogether unfit for the determination of the difference between the Sun's parallax and the parallax of *Venus* by the method on which reliance was exclusively placed in the treatment of the observations of 1769. I have not seen the papers of May 8, 1857, and June 10, 1864, in which this view is presented; but I am told

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that Lesson XLVI. of Mr. Lockyer's *Elementary Astronomy* is derived from the Astronomer Royal's papers. I conclude, therefore, that the consideration on which the view is founded is the following:—To determine the greatest difference in the length of the chords of transit, we must, if possible, select a place where both the ingress will be accelerated and the egress retarded, and another where the ingress is retarded and the egress accelerated. And I assume that the impossibility of finding two places which sufficiently fulfil these conditions in the case of the transit of 1874 is held to involve the failure of the method founded on the observed durations of transit.

It is unquestionably the case that no place can be found in northern latitudes which is at once near the point where ingress is most accelerated, and to the point where egress is most retarded. Nor can any spot be found near to the points where ingress is most retarded and egress most accelerated.

But this circumstance is counterbalanced by one which operates in another direction. In the case of the transit of 1874, the absolute maximum duration of transit, that is, the duration counted from ingress most accelerated to egress most retarded *for the whole earth*, differs very much more from the absolute minimum than in the case of the transit of 1882. For internal contacts the former difference exceeds the latter in the proportion of about three to two. This arises from the comparative shortness of the chord of transit in the case of the transit of 1874, and the increased effect which parallax consequently produces on its apparent length.

Hence it follows that although we cannot use anything like the full value of the difference in the case of the transit of 1874, we can yet obtain a resulting actual difference which is *fully equal* to that which we can obtain from the transit of 1882.

In order to test this view, I have made use of the following reductions for the effects of parallax, supplied with Mr. Hind's elements of the two transits:—

For the transit of *Venus* in 1874, the reductions for any place (taking ϱ = Earth's radius at the place; l = north latitude; and λ = latitude east of Greenwich), are

For Ingress.

External contact $-[2{\cdot}5621]\,\varrho \sin l - [2{\cdot}6872] \cos l \cos(\lambda - 137^\circ\ 1'.)$

For Egress.

External contact $+[2{\cdot}7213]\,\varrho \sin l + [2{\cdot}4833] \cos l \cos(\lambda - 38^\circ\ 20'.)$

In the case of the transit of 1882, the corresponding reductions are,—

For Ingress.

External contact $+[2{\cdot}5332]\,\varrho \sin l - [2{\cdot}4657]\,\varrho \cos l \cos(\lambda - 87^\circ\ 55'.)$

For Egress.

External contact $-[2{\cdot}2261]\,\varrho \sin l + [2{\cdot}6203]\,\varrho \cos l \cos(\lambda - 135^\circ\ 0'.)$

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I find that, if we take for the northern station in 1874, a place on the continent of Asia about 45° north latitude, and 120° east longitude; and for the southern station a place on Sabrina Land in 66° south latitude and 135° east longitude, there results a difference of duration of transit, equal to about $23^m\ 23^s$. By taking the southern station in about the same latitude and 105° east longitude (the place pointed out by the Astronomer Royal as suitable for the southern station in 1882), the difference becomes diminished to $22^m\ 44^s$. By taking Crozet's Island or Kerguelen's Land for the southern station the difference still continues large, though somewhat diminished. But by taking a place somewhat to the south-west of Nicolaief (close to Saghalien Island) for the northern station, and a place in 66° south latitude, and 135° east longitude for the southern station, the difference is increased to no less than $26^m\ 30^s$ (at the northern station the Sun would be low at the time of external contact at egress). The southern station might also be taken at Enderby Land (66° south latitude, and 50° east) without reducing the difference more than one minute.

Now in the case of the transit of 1882 the places selected by the Astronomer Royal, as being absolutely the most favourable so far as difference of duration is concerned, are, the Island of Bermuda and Sabrina Land (in longitude 105° east); and these places give a total difference of duration of $25^m\ 49^s$, nearly 41″ *less* than the greatest difference obtained above for the transit of 1874.

It will be seen also that the method founded on observed differences in the duration of transit has altogether the advantage over the other method; since we have a larger period of time to deal with by some minutes, and there is not in the case of the former method the same necessity for a scrupulously exact determination of the longitude of the place of observation.

Since the above was written I have re-examined the whole subject by a new method, with results strictly accordant with those above obtained. But additional inquiry into the circumstances of the transit of 1874, with special reference to the question of duration, and for the case of internal contacts, has led me to still clearer evidence of the value of this transit. I have constructed the orthographic projections of the Earth for the epochs of first and last internal contacts as seen from the Earth's centre. Across these projections I have laid down the lines which mark the order of the occurrence of internal contacts as seen from different parts of the Earth's surface, from minute to minute. Thus, in the first projection I have a series of twenty-six lines commencing with a tangent to the Earth's north-easterly quadrant, 133° 56′ from the north, and ending with a tangent to the Earth's south-westerly quadrant, 139° 26′ from the south: in the second I have a similar number of lines commencing with a tangent to the Earth's south-easterly quadrant 165° 59′ from

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the north, and ending with a tangent to the Earth's north-westerly quadrant 160° 4′ from the south. These lines enable one at once to recognise the best stations for observing the transit as most lengthened and as most shortened by parallax.

I find that any part of a nearly circular region extending from Lake Baikal to the southern part of Saghalien Island, and from north latitude 40° to north latitude 60° gives a well-lengthened transit, while the northernmost part of this region gives a lengthening of about $16\frac{1}{4}^m$. For southern stations there is a wide range of choice, though only three or four stations give nearly the full available shortening. I find, that Petra Island (assuming it rightly placed in my atlas, viz. in west longitude 88°, and south latitude 71°), gives a transit shortened by 20^m. The place near Repulse Bay mentioned by the Astronomer Royal as suitable for the observation of the transit of 1882, gives in 1874 a shortening of $18\frac{1}{2}^m$. The place near Victoria Land similarly referred to by Mr. Airy, gives a shortening of $18\frac{3}{4}^m$ in 1874. A place on Enderby Land will give a shortening of no less than $20\frac{1}{4}^m$. Crozet's Island and Kerguelen's Land are also suitable, giving transits shortened by 17^m and 16^m respectively. Macquarie Island, Royal Company Island, Hobart Town, and even parts of New Zealand, would serve as useful subsidiary stations.

The best southern station taken in conjunction with the best northern station gives a difference of duration of no less than $36\frac{1}{2}^m$. The maximum of difference in 1882 will be about 28^m. This is founded on the Astronomer Royal's statement that Sabrina Land and Bermuda give a difference which is to the absolute maximum of difference ($32\frac{3}{4}^m$ for internal contacts) as 341 to 400. Thus if we assume the value of a transit to be directly proportional to the observable difference of duration, the transit of 1874 is more valuable than that of 1882 in the proportion of $36\frac{1}{2}$ to 28, or more than 9 to 7. If, however, we hold that the slowness of ingress diminishes the value of a transit in precisely the same proportion that it increases the observable difference of duration, the transit of 1874 is inferior to that of 1882 in the proportion of

$$\frac{36\frac{1}{2}}{50\frac{1}{8}} \text{ to } \frac{28}{32\frac{3}{4}} \text{ or almost exactly 6 to 7.}$$

Probably the truth lies between these results, so that we may assign to the transit of 1874 a value which bears to that of 1882 the ratio of

$$\tfrac{1}{2}(9+6) \text{ to } 7, \text{ that is, } 15 \text{ to } 14.$$

Judged in either way the transit of 1874 is superior in value to that of 1769, *as actually utilized*.

In any case, it seems clear that the method founded on observed differences of duration is not inapplicable to the transit of 1874. On the contrary, when we remember all the difficulties in the way of the second method, the former seems to

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be the one from which the most satisfactory results are to be expected.

Mr. Stone has obligingly called my attention to papers in the *Comptes Rendus* by M. Puiseux, in which the duration of transit has been calculated for 9 places in 1874, with the Sun's elevation at the epoch of each phenomenon. I have not yet had an opportunity of examining this paper, or noting how far its author's object has been similar to that which I have had in the discussion of the results contained in the latter part of the foregoing paper.

Limits of Total Phase of the Solar Eclipse, 1869, August 7.
By J. R. Hind, Esq.

From $8^h\ 51^m$ to $11^h\ 9^m$ G.M.T.

G.M.T.	Northern Limit. Long.	Northern Limit. Lat.	Central Line. Long.	Central Line. Lat.	Southern Limit. Long.	Southern Limit. Lat.	Dur. of Total Phase on Cen. Line.
h m	° ′	° ′	° ′	° ′	° ′	° ′	m s
8 51	135 41 E.	59 7 N.	140 12 E.	59 2 N.	144 1 E.	58 47 N.	2 35
8 53	140 59	60 22	144 53	60 3	148 20	59 39	2 41
8 55	145 31	61 20	149 4	60 53	152 14	60 21	2 46
8 57	149 38	62 8	152 55	61 34	155 52	60 56	2 51
8 59	153 29	62 47	156 32	62 7	159 17	61 25	2 55
9 1	157 9	63 20	159 59	62 36	162 31	61 50	2 59
9 3	160 39	63 48	163 17	63 0	165 38	62 10	3 2
9 7	167 15	64 31	169 31	63 36	171 33	62 40	3 10
9 11	173 26	65 0	175 23 E.	64 0	177 5 E.	63 0	3 16
9 15	179 17 E.	65 16	179 7 W.	64 12	177 44 W.	63 9	3 21
9 19	175 10 W.	65 22	173 54	64 15	172 48	63 10	3 26
9 23	169 55	65 19	168 58	64 10	168 8	63 3	3 30
9 27	164 55	65 9	164 16	63 59	163 42	62 51	3 34
9 31	160 12	64 50	159 49	63 41	159 30	62 32	3 38
9 35	155 44	64 27	155 37	63 17	155 31	62 8	3 41
9 39	151 31	63 58	151 37	62 48	151 44	61 40	4 43
9 43	147 32	63 24	147 51	62 15	148 8	61 8	3 45
9 47	143 46	62 47	144 16	61 39	144 43	60 32	3 47
9 51	140 12	62 5	140 51	60 59	141 28	59 53	3 48
9 55	136 49	61 20	137 37	60 15	138 22	59 11	3 48
9 59	133 36	60 32	134 32	59 28	135 24	58 26	3 49
10 3	130 33	59 41	131 35	58 39	132 33	57 38	3 49
10 7	127 38	58 47	128 46	57 47	129 49	56 48	3 49
10 11	124 50	57 50	126 3	56 53	127 11	55 55	3 48
10 15	122 7	56 52	123 25	55 56	124 38	55 1	3 47
10 19	119 30	55 51	120 52	54 57	122 9	54 4	3 45

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From $8^h\ 51^m$ to $11^h\ 9^m$ G.M.T.

G.M.T.	Northern limit. Long.	Lat.	Central Line. Long.	Lat.	Southern Limit. Long.	Lat.	Dur. of Total Phase on Cen. Line.
h m	° ′	° ′	° ′	° ′	° ′	° ′	h m
10 23	116 57	54 48	118 22	53 56	119 42	53 5	3 43
10 27	114 27	53 42	115 55	52 53	117 18	52 4	3 41
10 31	111 58	52 34	113 30	51 47	114 56	51 0	3 38
10 35	109 31	51 23	111 5	50 39	112 34	49 54	3 35
10 39	107 2	50 9	108 40	49 27	110 10	48 45	3 31
10 43	104 32	48 53	106 12	48 13	107 46	47 33	3 27
10 47	101 57	47 32	103 40	46 56	105 18	46 19	3 22
10 51	99 15	46 8	101 2	45 34	102 42	45 0	3 16
10 55	96 23	44 39	98 13	44 8	99 58	43 37	3 10
10 59	93 14	43 2	95 12	42 36	97 3	42 9	3 4
11 1	91 31	42 11	93 34	41 27	95 28	41 22	3 0
11 3	89 41	41 16	91 49	40 55	93 48	40 32	2 56
11 5	87 40	40 18	89 56	40 1	92 0	39 40	2 52
11 7	85 26	39 16	87 51	39 3	90 2	38 45	2 48
11 9	82 52 W.	38 6 N.	85 31 W.	37 59 N.	87 52 W.	37 46 N.	2 44

Nautical Almanac Office,
1869, *March* 1.

Occultations of Stars observed at Waterloo, near Liverpool.
By John Joynson, F.R.A.S.

1868.			G.M.T. h m s	Nautical Almanac. h m	Angle given. °	
Dec. 27	B.A.C. 1526	Reap.	7 25 36·0	7 22	318	± 3m later, as it should be.
1869. Jan. 23	Aldebaran	Dis.	8 44 15·8	8 49	97	± 5m earlier; should be later.
		Reap.	±10 0 0	10 5	289	± 5m earlier; should be later.
24	115 Tauri	Dis.	5 27 5·0	5 29	52	± 2m earlier, as it should be.
		Reap.	6 16 38·6	6 12	333	± 3m later ,,
	119 Tauri	Dis.	8 17 25·7	8 14	143	± 3m later ,,
		Reap.	9 0 44·6	9 10	237	± 9m earlier ,, but hardly so much.
	120 Tauri	Dis.	8 46 27·4	8 48	117	± 2m earlier, should be later.
		Reap.	9 48 13·3	10 0	260	± 12m earlier, as it should be, but hardly so much as 12m ?

Lat. 53° 28′ 24″ N.
Long. $0^h\ 12^m\ 7^s$ W.

The reappearance of *a Tauri* (*Aldebaran*) could not be got exactly for passing cloud; but it was seen at $10^h\ 1^m$ G.M.T. about 75 seconds of arc from the limb; so that it must have reappeared rather before 10 P.M.

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The times got on the 24th were deemed exact in each case, though in some cases they differ more than I think they should from the times stated in the *Nautical Almanac*.

Waterloo, Feb. 5, 1869.

Note on Personality in the Determination of the Line of Collimation of a Transit. By E. J. Stone, Esq.

A personality has been noticed between some observers in the determination of the line of collimation of a Transit instrument. The existence of such a personality to a sensible amount would be extremely difficult to deal with in a perfectly satisfactory manner: it would cast a certain amount of doubt over the whole of the instrumental corrections. I have thought it worth while, therefore, to determine whether such a personality exists between the Greenwich observers. The observations at Greenwich are chiefly made by Messrs. Dunkin, Ellis, Creswick, and James Carpenter. The collimation error of the Transit-circle is determined on every day except Sundays. It is usual, unless there are indications of a real change, to adopt the mean of the readings made during a week as the true reading for the line of collimation to be used in reduction of the observations made during the week. We shall have, therefore, in many cases, a collimation reading made by Messrs. Dunkin, Ellis, Creswick, and Carpenter, all within a week, and generally two collimation determinations by one or other of these gentlemen within the week. These duplicate determinations are valuable for testing the stability of the line of collimation during the week. I have adopted the mean of such duplicate determination for the observers' reading for the week's comparison. In the last two years I have found 38 of these complete weekly comparisons. These 38 comparisons, split up into three sections of 11, 16, and 11 comparisons, give results so closely agreeing with the means of all, that I have not thought it necessary to extend the comparisons.

The following are the corrections to the adopted readings, as determined by Messrs. Dunkin, Creswick, Ellis, and Carpenter:—

		r
Correction from	Mr. Dunkin's Observations	+0·021
	Mr. Ellis' ,,	+0·024
	Mr. Creswick's ,,	+0·014
	Mr. Carpenter's ,,	+0·018

or, taking the deviation from the mean as error of each observers' method, we have

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John Browning, del.

TRAIN OF SUN-SPOTS.

Fig. 1.

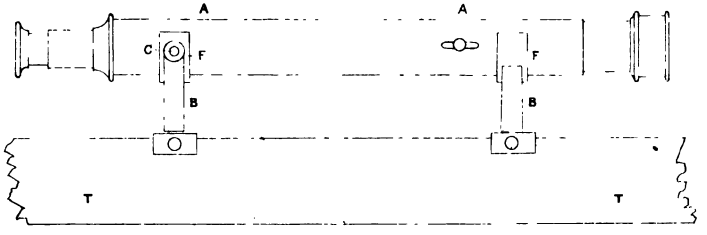


Fig. 2.

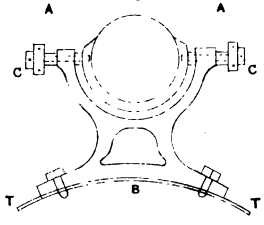


Fig. 4.

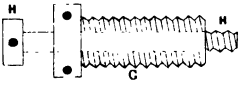
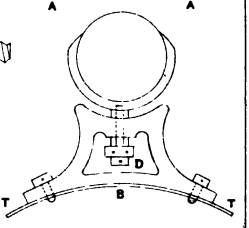


Fig. 3.



NEW MOUNTING FOR FINDER.

John Browning, del.

Malby & Sons, Lith.

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For Mr. Dunkin	$+0^{s}\cdot002$	=	$0''\cdot030$
Mr. Ellis	$+0^{s}\cdot005$	=	$0''\cdot075$
Mr. Creswick	$-0^{s}\cdot005$	=	$-0''\cdot075$
Mr. Carpenter	$-0^{s}\cdot001$	=	$-0''\cdot015$

The greatest difference is between Mr. Ellis' and Mr. Creswick's determinations, but these differ by only $0''\cdot15$.

This quantity, small as it really is, is real. Each of the three sub-groups gives a result of the same sign, agreeing closely in amount, and an independent comparison of 51 consecutive readings of C. and E. gives a similar result. The magnitudes of these discordances are so small, that they may fortunately be most safely neglected. The result appears to be, that, so far as Greenwich is concerned, the uncertainties introduced into the collimation determinations by personality are insignificant.

On an extensive Train of Sun-spots. By John Browning, Esq.

About noon, on the 7th of March, I saw a long train of Sun-spots through the finder of my Equatoreal.

Attempting to observe them with my 12-inch silvered-glass reflector, I found the definition so bad that I was compelled to reduce the aperture to 6 inches before I could obtain tolerably steady vision. I then fitted a solar plane, that is a plate of parallel glass silvered on the exposed surface, on to the mouth of the telescope.

With a parallel-wire micrometer I measured the spots, and made the accompanying drawing to scale, see Plate.

They presented the appearance of an almost continuous penumbra, in an irregular hollow curved line, with umbræ at intervals. Some of these umbræ contained blacker nuclei.

On the western side a portion of the penumbra assumed the form of a pair of compasses.

I found the outside dimensions of the cluster to be from E. to W. 97,700 miles, and from N. to S. 27,130 miles. This is by far the largest group of spots I have seen during the recent outbreak. Taking a mean line through the group, they were almost exactly parallel to the Sun's equator.

A few bright bridges crossed some of the penumbræ, but no fine stipplings such as I have mostly seen were visible on them. The air was, however, so unsteady that I could only use a power of 127. A higher power than this would probably have been necessary to make them perceptible. The coarse mottlings were easily seen all over the solar surface.

In the case of some of the previous spots I observed that when they were near the centre of the solar disk but few bright faculæ could be seen surrounding the penumbra. As these spots

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approached closely to the Sun's limb the faculous matter became brighter and brighter, until when the spots had reached the limb it quite obliterated the penumbra. The black spots were then seen surrounded by a bright border. This is a proof, if any is needed, that the faculæ are far above the solar surface.

On an Improved Method of Mounting Finders.

By John Browning, Esq.

The manner of mounting a finder may seem too small a matter to bring before the Royal Astronomical Society, yet when it is considered that a valuable observation may be lost merely from a finder being out of adjustment, it will at once be admitted that the matter is one well worthy of attention.

Professor Piazzi Smyth wrote me a short time since "that he thought some better plan for mounting and adjusting finders was much required." This induced me to devise the contrivance I shall presently describe.

Two plans are in general use for adjusting finders, both of which have obvious disadvantages. In the first the finder is placed in the centre of two rings somewhat larger than itself, and adjusted by means of three screws working through each ring, and pressing on bands which serve to strengthen the finder. This adjustment is liable to the following objections:—The rings prevent a line of sight being taken along the body of the finder; the screw-points are apt to shift their position on the rings, even if resting in groves, should the observer inadvertently attempt to direct the telescope by means of the finder. Like all three-screw adjustments, several trials have generally to be made before an error can be rectified.

The second plan consists in attaching the finder to the body of the telescope as a fixture, on two uprights. The adjustment in this case is made by moving a stop carrying the cross-wires placed in the focus of the eye-piece, by means of screws which pass through the body of the finder. In most respects this plan is good, but it has the serious objection of considerably limiting the field of view. Of course the largest attainable field of view is required in a finder.

The plan figured in the Plate seems to me to avoid the whole of the foregoing objections.

Fig. 1 represents the finder (A A) mounted by the aid of the uprights (B B) on the telescope (T T). The finder has two semicircular bands (F F) round the lower half of the body. The upright (B) has two hollow gun-metal screws (C C), which work in bosses at the sides. These screws press against the stout semicircular band (F), and serve to move the finder horizontally. Two smaller steel screws pass through the gun-metal screws, and are themselves screwed into the band (F) on the finder. The con-

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struction of these double screws is shown in fig. 4, where G is the hollow screw, the end of which acts against the band, and (H) the central screw which clamps into the finder.

Fig. 2 is a section of the finder and upright, as well as the clamping and adjusting screws, at the point B.

The vertical adjustment is effected by means of a similar pair of screws, as at D, fig. 3.

It will be evident from the construction that when the finder is being moved vertically it will swivel slightly on the horizontal screws, C C, while, when it is being moved horizontally, it will swivel on the vertical clamping screw, D, fig. 3; thus no undue strain will be thrown on any part.

I have the honour of exhibiting a 2-foot finder mounted in this manner, and I think a trial of its firmness will convince any one that it would not be possible, except by breaking or bending some of its parts, to put it out of adjustment.

When using a star-spectroscope attached to a telescope it is necessary to bring the star or other object under examination exactly between the jaws of the slit of the spectroscope, *or no spectrum will be visible.* This is a task of exceeding difficulty, unless the finder on the telescope will remain accurately in adjustment. For such a purpose a finder mounted as described would be valuable.

The Johnson Memorial Prize.

The Trustees of the Johnson Memorial Prize for the encouragement of the study of Astronomy and Meteorology, propose the following subject for an Essay:—

"*On the Laws of Wind.*

"1. With regard to Storms.

"2. With regard to average Periodical Phænomena at given places on the Earth's Surface."

The Prize is open to all Members of the University of Oxford; and consists of a Gold Medal of the value of Ten Guineas, together with so much of the dividends for four years on 338*l.* Reduced Annuities as shall remain after the cost of the Medal and other expenses have been defrayed.

Candidates are to send their Essays to the Registrar of the University under a sealed cover, marked "Johnson Memorial Prize Essay," on or before the 31st day of March, 1871, each Candidate concealing his name, distinguishing his Essay by a motto, and sending at the same time his name sealed up under another cover with the same motto written upon it.

No Essay will be received after the 31st day of March, 1871.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIX. *April* 9, 1869. No. 6.

Admiral MANNERS, President, in the Chair.

The Rev. S. J. Perry, of Stonyhurst College,

was balloted for and duly elected a Fellow of the Society.

On the Rotation of Mars. By Richard A. Proctor, B.A.

Mr. Browning having mentioned to me several months ago, that he hoped to be able to obtain several drawings of *Mars* at the present opposition, by means of his 12-inch reflector, it occurred to me that I might avail myself of so favourable an opportunity to test the estimate I had formed of the rotation-period of *Mars*. It is true that, when an estimate is formed from the rotations occurring in the course of so long a period as 201 years, one cannot expect any important correction to be obtained by throwing in the rotations of another biennial period. But there was this to be considered:—The drawings of *Mars* which I had used in the former calculations had not been constructed with any reference to a computation of this sort. Those who are familiar with the difficulties inherent in the observation of *Mars* will recognise the fact that the draftsman might very well place a feature slightly on one side or the other of its true position. I have found this to be the case in drawings by the best observers, at least if the hour and minute appended is to be considered the true epoch at which the drawing is made. On the other hand, a simple observation having reference only to the central passage of some distinct marking is a matter about which there is much

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less chance of mistake. The experiments of Sir Wm. Herschel as to the views which even unpractised observers form respecting the centering of a disk show how little chance of error there is in a matter of this sort; and in the case of a practised observer like Mr. Browning the chance of error was much diminished.

The special work, then, which I asked him to favour me by carrying out was the observation of a central passage of the Kaiser Sea (using the nomenclature of my chart of *Mars*) — that is, the long funnel-shaped sea running north and south, sometimes called the Hour-glass Sea. This feature is depicted in two views of *Mars* taken by Hooke on March 12th, 1666 (New Style), at $12^h\ 20^m$ and $12^h\ 30^m$; and my former estimates of *Mars'* rotation period had been obtained by comparing Hooke's views with drawings of the same feature by Herschel, Beer and Mädler, Dawes and Browning. From the close agreement between the two drawings taken by Hooke and the true figure given to the Kaiser Sea, I am disposed to place much more reliance on his observations than one would ordinarily accord to the work of the astronomers of the seventeenth century. There can be no doubt that he saw the Kaiser Sea under peculiarly favourable circumstances on that spring night in 1666, since his drawings will bear favourable comparison with any that have been obtained in recent times with instruments of moderate power.

So much for the observation which limits one end of the long period.

The observation next to be considered is the one which Mr. Browning obligingly made at my request. According to this observation the Kaiser Sea was centrally situated on the disk of *Mars* on February 4, at $11^h\ 3^m$ G. M. T. A few minutes before this hour Mr. Browning felt certain that the sea was to the left of the centre, and a few minutes after he felt certain it was to the right. This will be understood when we remember that a point near the middle of the disk traverses $\frac{1}{470}$th of the disk's diameter in a minute. So that if we take a range of eight minutes on either side of the epoch of central passage, we get a range of motion equivalent to about $\frac{1}{30}$th part of the disk's diameter, and there would be no mistaking so considerable a motion as this.

At the hour of observation the longitude of the Earth was about $135\frac{1}{2}$ degrees; that of *Mars* about $141\frac{1}{2}$ degrees. Marking down the points thus indicated upon my chart of the orbits of the four inner planets, and drawing in also the points occupied by the two planets at the epoch of Hooke's observation, I found the difference of *Mars'* geocentric longitude to be about $35\frac{1}{2}$ degrees. From the care taken in the construction I think this result may be trusted within a quarter of a degree at the outside. It results that the correction for geocentric longitude may be obtained by adding the period in which the planet rotates through an angle of $35\frac{1}{2}$ degrees, or by subtracting the period of rotation through $324\frac{1}{2}$ degrees.

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Next, as to the correction for phase. It is obvious from Hooke's first drawing—see *Monthly Notices* for January 1868—that the Kaiser Sea was some 18 degrees (or perhaps rather more) from central passage at the hour of observation. Thus we must deduct a further interval equivalent to the period of the planet's rotation through somewhat more than 18 degrees, because Browning observed the planet when rotated so many degrees further than when seen by Hooke.

We have then, in all, a deduction equal to the period of the planet's rotation through $324\frac{1}{2}^\circ + 18\frac{1}{2}^\circ$ (say) $= 343^\circ$. This corresponds to a deduction of

$$\frac{343}{360} \times 88643 \text{ seconds,}$$

or

84457 seconds.

Now the total number of seconds between

March 12th, 1666, $12^h\ 20^m$,

and

February 4th, 1869, $11^h\ 3^m$,

will be found to be

6402926580 seconds,

and the corrected interval is therefore,

(6402926580 − 84457) seconds
= 6402842123 seconds.

The number of rotations obtained by dividing this period by a rough estimate of the rotation-period at 88642·7 seconds is found to be 72232.

Lastly, dividing 6402842123 by 72232, we obtain for the rotation-period the value

88642·736 seconds,

or

$22^h\ 37^m\ 22{\cdot}736$ seconds,

the value named in my last paper on the subject being

$22^h\ 37^m\ 22{\cdot}735$ seconds,

and the limits of probable error

0·005 seconds.

The three intervals before examined, ranging from 190 to 201 years, gave severally

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88642·737 seconds
88642·734 ,,
88642·734 ,,

The interval now considered, one of nearly 203 years, gives a value between the greatest of these and the two others; and as all the values now obtained lie between

88642·73 s. and 88642·74 s.

we may assume that the planet's rotation period is fairly represented by the value

$22^h\ 37^m\ 22{\cdot}735$ seconds,

with a probable error of ·005 and a possible error of ·015ˢ. More than this error, which gives a range of ·03ˢ for each rotation, and therefore $36^m\ 6^s{\cdot}96$ for the total number of rotations since the date of Hooke's observations, it would be unreasonable to admit, since the planet's change of appearance, even in a quarter of an hour, is perfectly cognisable by a practised observer.

Kaiser's period ($22^h\ 37^m\ 22^s{\cdot}62$) which differs from mine by $0^s{\cdot}115$, would throw the planet out, at the period of Hooke's observation, by an amount corresponding to the amount of rotation during $2^h\ 20^m$. This is not to be thought of for a moment. Hooke depicts the Kaiser Sea at a distance of rather more than 18° from the centre, and the change resulting from Kaiser's period would throw the Kaiser Sea nearly 50° from the centre. At this distance it would have been lost in the haze near the planet's limb, even had Hooke's telescope been as powerful as the best modern instruments. On the other hand, the fact that Kaiser was led to the period $22^h\ 37^m\ 22^s{\cdot}62$ by the comparison of some of the best modern drawings, taken over a widely extended period, suffices to prove that Hooke's drawing depicts the Kaiser Sea, and not the Dawes' Strait, which sometimes looks not unlike the Kaiser Sea, especially at that season of the Martial year in which Hooke observed the planet. For Kaiser's period throws the Dawes' Strait almost exactly in the centre of that hemisphere of *Mars* which would have been turned away from Hooke at the time of his observation.

The period deduced by Messrs. Beer and Mädler, $22^h\ 37^m\ 23^s{\cdot}8$, is disproved by Sir W. Herschel's drawings; otherwise it would fairly account for Hooke's drawing on the omission of a complete rotation. Herschel's views coming midway between Hooke's and modern drawings satisfactorily remove any doubt of this sort.

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On the Meteoric Shower of November, 1868, *as seen at the Royal Observatory, Cape of Good Hope.* By Geo. W. H. Maclear, Esq.

I have the honour to forward for the information of the Royal Astronomical Society, the result of the watch kept up at this Observatory, on the 13th, for the November meteoric shower.

The first meteor from *Leo* was noted at $13^h\ 18^m\ 30^s$, Cape mean time.

Shower at its maximum about $14^h\ 42^m$.

Last meteor noted (in broad daylight) at $16^h\ 27^m\ 20^s$.

The total number observed amounted to 134, but a dense haze prevailing over the eastern horizon doubtless obscured many others from view. For the same reason we were unable to observe the exact radiant point. Tracing back some of the brighter ones, it appeared to be between γ and ζ *Leonis*, and a little to the north of γ.

No meteors were noted on the 12th of November.

On the 14th the sky was clouded.

Meteoric Shower of November 13th, 1868.

Cape Mean Time. h m s	Direction. From	To	Remarks.
13 18 30	Leo	Zenith	White, large
27 40	,,	Westward	Orange and greenish, large
34 35	Hydra	E. horizon	Small
36 32	γ Leonis	S.	Faint
49 0	Leo	Zenith	White and green
52 5	,,	S. along horizon	,,
57 50	α Hydræ	Horizon	Small
57 50	,,	,,	,,
13 58 5	γ Leonis	Horizontal to S.	,,
14 1 45	η Leonis	Zenith	
3 5	,,	S.	
6 10	,,	S.W.	
7 45	,,	,,	
11 30	Leo	S.	
12 10	Below Regulus	Horizon	Blue and green
14 30	Regulus	S.	Faint
15 20	,,	,,	,,
15 50	,,	,,	,,
17 20	Above Regulus	Horizon	,,
18 35	Leo	S.W.	Orange, large
20 27	,,	Horizontal to S.	
22 20	,,	,,	Small, no train
22 32	,,	,,	Large
23 10	,,	Zenith	,,

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Cape Mean Time. h m s	Direction. From	To	Remarks.
14 23 33	Leo	S.	
24 47	Below Leo	Horizontal to S.	Small
25 0	,,	,,	
25 40	,,	,,	
26 50	,,	,,	
28 10	,,	,,	
30 58	,,	,,	
32 45	,,	,,	
33 55	,,	,,	
34 15	Below ♂	Horizon	
35 30	Regulus	S.W.	Orange, large
36 5	μ Leonis	Horizon	Blue, "pear" shaped
36 30	,,	S.	
36 50	,,	,,	
37 5	,,	,,	
37 24	Regulus	,,	Long train
40 35	Below Regulus	,,	Small
41 50	Leo	,,	
42 0	,,	,,	
42 3	,,	,,	
42 12	,,	Horizontal to S.	
42 12	,,	,,	
42 13	,,	Zenith	
43 30	,,	Horizon	
44 20	S. of Regulus	S.W.	Head, orange; train, blue an white. Very fine
45 57	Leo	Horizontal to S.	Bright
46 32	,,	S.	Orange
49 50	,,	,,	,,
49 50	,,	,,	
50 5	,,	N.	
50 50	,,	S.	
53 43	N. of ♂	Zenith	Head, orange; train, blue
54 10	Leo	Horizon	
55 12	,,	S.	
55 20	,,	,,	
56 10	,,	Horizon	"Pear" shaped.
57 45	Below Regulus	S. W.	
57 52	,,	,,	
58 28	Under ♂	N.	
58 40	Leo	S.	Small
14 59 5	,,	,,	
15 4 7	ζ Leonis	Horizon	"Pear" shaped.
4 27	,,	,,	

maximum.

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Cape Mean Time. h m s	Direction. From	To	Remarks.
15 4 35	♂	N.	
6 5	Leo	Horizon	
7 40	,,	Zenith	
10 45	,,	Horizontal to S.	Very bright.
11 50	♂	N.	
12 5	♀	S.	
13 43	♀	Regulus	
13 50	Regulus	S. W.	
14 30	,,	S.	Long train.
15 15	,,	Zenith	,, orange.
15 50	Below ♂	Horizon	Large.
16 50	,,	S.	
19 30	,,	Horizon	
20 0	,,	N.	
21 35	,,	S.	Orange.
25 8	,,	N	
25 11	,,	,,	
27 1	ι Leonis	♂	
27 10	ϑ Leonis	Horizon	
27 55	,,	N.	
28 0	,,	S.	
28 50	ζ Leonis	N.	
29 40	β Leonis	♀	
29 50	ϑ Leonis	S.	
30 4	,,	W.	Red train.
30 10	α Hydræ	Horizon	
30 55	β Leonis	N. horizon	
34 10	β Corvi	Horizon	
34 30	ϑ Leonis	,,	
36 10	η Leonis	β Corvi	Orange.
36 35	ϑ Leonis	Horizon	
36 35	♂	W.	
37 10	Regulus	S. W.	Orange, large.
37 25	,,	α Centauri	Large, long train.
39 55	γ Leonis	W.	Orange and red, bright.
41 10	♂	Horizon	
41 30	η Leonis	β Corvi	Small.
41 50	ζ Leonis	S. W.	
41 55	Regulus	,,	
42 15	ζ Leonis	Horizon	
42 40	,,	S. W.	Orange blue.
43 5	β Leonis	S.	
15 43 25	Leo	E. Horizon	Orange, train green.

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Cape Mean Time. h m s	Direction. From	To	Remarks.
15 43 30	Regulus	α Centauri	,,
43 50	,,	,,	,,
43 52	,,	,,	,,
46 15	δ Leonis	β Corvi	
46 17	,,	,,	
46 50	,,	,,	
46 55	,,	,,	
47 0	Regulus	α Centauri	Orange, large.
49 30	ζ Leonis	Horizon	Orange and green.
49 40	,,	,,	,,
50 30	δ Leonis	,,	Small.
51 10	,,	,,	Green, small.
51 15	,,	S.	,,
52 10	,,	β Corvi	Orange and Green.
53 50	ζ Leonis	Horizon	
55 25	,,	W.	,,
57 15	Regulus	S.W.	Small.
58 7	η Leonis	,,	,,
15 59 0	ζ Leonis	Horizon	,,
16 4 0	Regulus	S.W.	
7 40	δ Leonis	Horizon	Orange.
12 0	,,	Spica	Orange and Green.
26 40	Regulus	β Corvi	
16 27 20	δ Leonis	Spica	

(Total 134.)

Royal Observatory,
Cape of Good Hope, Nov. 17th, 1868.

On some points connected with the Rediscussion of the Observations of the Transit of Venus, 1769. By E. J. Stone, Esq.

At the Séance de l'Académie des Sciences, 1869, January 4, M. Faye brought forward a defence of M. Powalky's discussion of the observations of the transit of *Venus*, 1769. The weight which, irrespective of its merits, is necessarily attached to a paper read before so learned a body and by so distinguished an Astronomer as M. Faye, compels me at once to express the grounds on which I am unable to accept either the methods or the results of M. Powalky's discussion.

My first inquiry shall be respecting the foundation of M. Faye's remark, that M. Powalky "a eu le mérite d'élucider le premier la cause d'une erreur compromettante pour la science."

I will lay down at once a few propositions to which I shall have frequent necessity of appealing.

1. There are two phenomena at internal contact which were

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observed in 1769. The so-called real internal contact, and the apparent internal contact. These phenomena were in the transit of 1769 separated *roughly* by about 16ˢ.

2. An observation of an apparent contact is not an observation of a real contact, and *vice versâ*.

3. In any *legitimately* conducted investigation, the use of an apparent contact for a real contact, or *vice versâ*, must lead to an erroneous value of solar parallax to the extent which an arbitrary change of about 16ˢ in the incongruous observation would affect the final result.

4. If observations have to be rejected because of their incompatibility with the general run of the discussion, then every such rejection enormously increases the presumption against the truth of the investigation which requires such rejection.

5. The absolute necessity of rejecting *one* good observation would, in itself, be conclusive evidence against the truth of the deduced value of solar parallax or the logic of the method applied.

M. Powalky attributes the difference between his value and that deduced by Encke to his employment of more accurate longitudes than those available to Encke. The reason why I am compelled to dissent from this assertion will best appear by a consideration of the observations made use of by M. Powalky at those stations where durations were observed. We shall thus clear ourselves entirely from the effects of these more accurate determinations of longitude.

Wardhus Station.

M. Powalky employs, for internal contacts, Sajnovics' observation at ingress, "Certissimus ut ajebat." At egress, Hell's observation, "videtur aliqua gutta nigra intra limbum Solis et Veneris ante contactum fieri."

We have here one violation of (3).

Kola.

The observations at this station are not employed by M. Powalky.

Hudson's Bay.

We have here two observed durations from real contact to real contact. M. Powalky employs both observations at ingress, which are well represented in his residuals. He rejects, on account of discordance, both observations at egress. The observed durations agree within 1ˢ. We have here two violations of (4) if not even of (5). M. Powalky's solution would make each of the observers to have caught the real contact at egress *about* 13ˢ too soon.

St. Joseph.

M. Powalky has used the three observations of Chappe, V. Doz, and Medina, as observations of real contact. He has, however,

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only given to the three observations the weight of one. The residual of the mean of the three observations at ingress amounts to $-7^s{\cdot}5$.

Otaheite.

M. Powalky follows Encke in considering the observations of apparent contact of the "penumbra as dark or nearly as dark as the planet itself," as observations of real contacts.

He employs, at ingress, the observations of Green and Solander. He rejects Cook's observation, with a residual of $-15^s{\cdot}6$. At egress, M. Powalky uses Green's observation with a weight only of $\frac{1}{2}$: the residual is $-7^s{\cdot}8$. He rejects Cook's observation, with a residual of $-17^s{\cdot}8$.

Now, upon this I would remark that, if the observations are observations of real contact, M. Powalky has here, by the rejection of Cook's observations and the little weight attributed to Green's observation at egress, violated (4), if not (5), several times. If these observations are not observations of real contacts, M. Powalky has violated (3) by using Green's observations. In these durations, be it remembered, we are free from the effects of longitude.

Of the ten complete durations from internal contacts (all that are available) which I have used in my paper and which are there, at least, perfectly represented, M. Powalky has employed, in their entirety, only four. To three of these combined he gives the weight of one only. The fourth duration, that at Otaheite, he partially rejects by giving only a weight of one-half to Green's observation at egress.

To these he has added a duration obtained from an ingress observation of Sajnovics' and an egress observation of Hell. This is the only duration well represented in his solution.

With respect to the observations at stations where durations were not observed I shall have but little to remark.

The selection of Hitchins' observation at $7^h\,28^m\,57^s{\cdot}6$ A.T. as the representative observation of real contact, in opposition to the whole body of observers in England and Southern Europe, appears to me unsound. If we know better than these observations can teach us, we need not appeal to them, but if we do appeal to them for information we must accept fairly the information they afford. The greater the discordances amongst the observations—the greater the difficulties under which the observations were made—the more unsafe must become any selection of an extreme value, like that of Hitchins', to the exclusion of the mean.

The selection of Shippen's observation as the representative observation of real contact for observations made with factor about 27, on the American continent, appears to me also objectionable. There are at least seven other observations on this station, all of which appear to give a later time than Shippen's for the disappearance of the connecting ligament at ingress.

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An alteration in the value of solar parallax of 0″·4 is a question only of less than 2^s between Wardhus and Greenwich, and of less than 7^s between Wardhus and Shippen's station, Philadelphia. The observations of Hitchins and Shippen, selected from groups, and each early and extreme observations of real contact, are given full weight. I think it will hardly escape notice how M. Powalky's solution fails, where the real strain falls upon it, to reconcile the St. Joseph, and more particularly the Otaheite observations with those made in the extreme northern stations. I must remark, M. Powalky has considered the question of adopted semi-diameter of so little importance in the discussion, that he has adopted Encke's value deduced from the employment of very different data.

We knew, before M. Powalky's paper appeared, that by adopting different data different results could be obtained. What we did not know was any cause for these discrepancies. The selection of the material had been based upon no intelligible principle, and no one had been able to reconcile the whole of the durations. I now leave M. Faye's defence of M. Powalky's work to the judgment of competent astronomers. I, at least, do not consider that "il a eu le mérite d'élucider le premier la cause d'une erreur compromettante pour la science."

I now pass to my own work upon this important question. In my opinion all the difficulties which have been experienced in the discussion of the observations of the Transit of *Venus* in 1769, are to be traced to a single source—mistaken notions with respect to the adoption of semi-diameter, and insufficient recognition of the nature of the uncertainties thus introduced. My view will perhaps be more clearly understood as follows. If we knew the true value of the solar parallax, the true R.A. and N.P.D. of the Sun and of the planet, the true semi-diameter of the Sun and planet, we could compute, with the utmost accuracy, the time of *real* contact for each station. If we knew the apparent semi-diameters of the Sun and of *Venus*, when projected upon the Sun's disc, and enough of the theory of irradiation, we could compute with the same accuracy the apparent internal contact; in fact, every phase of the phenomena presented from the real contact to the exterior contact. Now, the so-called observed real internal contacts which are the times of first appearance or disappearance of the connecting ligament (*i.e.* the times at which the observer first recognises the existence of the ligament) can no more, at present, be directly computed than the so-called apparent contacts. The two phenomena stand in much the same position. They are both complicated by the effects of irradiation. If we make use of durations, every observed duration, either from apparent contacts or real contacts, will lead to an equation of condition of the form $\frac{x_1+y_1}{2} + a_1\, d\pi =$ observed discordance; a_1 is a constant depending on the observer's position, $d\pi$ the correction to the assumed parallax, x_1 and y_1, two functions of errors of relative

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tabular R.A. and N.P.D., and of *correction for semi-diameters, corresponding to the observed phase at ingress and egress respectively.*

Now the observer really observed *some phase.* If we knew the true values of x_1 and y_1, each equation would give the true value of $d\pi$. Unfortunately we know nothing about x_1 and y_1. Their values must be deduced from the observations themselves as approximately as we can find them. If we can find several observations which appear to be observations of the same phase, we can assume the corresponding x and y, one or both, to be the same for these several observations. The deviation of the phase really caught by the observer from our mean phase is now really the so-called error of observation, which is, in fact, nothing more than the error committed in assuming that the phase observed is the mean phase.

The attention of observers having been chiefly directed to observations of apparent contact and the so-called real contacts, the observations naturally group themselves around two mean phases near the time of real contact and the time of apparent contact respectively. The observations of these two phases differ by about 16^s. If we had enough observations, we might assume all these contacts, real and apparent, to be observations of the same phenomenon. The probable error of an observation measured by the residual errors would in this case be very large, for we include in these residuals all the errors made by assuming that the observations refer to the mean phase. A very large number of observations well distributed would thus be required in order that a result of any accuracy should be deduced. We clearly have not enough observations for such rough work.

If we advance one step beyond this, and assume that all the observations of real contact correspond to one mean phase, all the observations of apparent contact to another mean phase, we shall have made a considerable reduction in the probable error of each observation. We shall thus require, comparatively speaking, a small number of observations, even although we have thus introduced one more unknown quantity. This, of course, depends upon the attention of the observers having been directed to two essential points; the observer's attention having been directed to the real and apparent contacts, the observations naturally group themselves around two corresponding mean phases. I have, therefore, adopted two unknown quantities x and y; one corresponding to an apparent, the other to a real mean phase. And here a word or two on M. Faye's assumption, with respect to the accuracy which is thus to be obtained. That accuracy depends only, upon the factors of parallax correction involved in the equations of condition and the closeness with which the observations of apparent and real contacts group themselves around their respective mean phases. The accuracy really obtained can be judged of solely by the smallness of the final residuals. It may, perhaps, be said, that in thus

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introducing one more unknown quantity, the final residuals must necessarily be smaller than those otherwise obtained. To this I reply that the new unknown quantity which I have introduced amounts to very little more than the arbitrary selection of observations which were assumed to be real internal contacts at those stations where both apparent and real contacts were observed, a thing which has always been previously done. At all events, I use both apparent and real contacts, and I have introduced no more unknown quantities than are absolutely required.

There is one important point that must here be borne in mind. The x and y introduced in my equations of condition are determined by making the residual errors the least possible. This is the analytical consideration from which they are determined. But x and y have been introduced to correspond to the observed mean phases of apparent and real contact. They have, therefore, a physical interpretation. We must, therefore, have, if our solution is a true solution, $\frac{x-y}{2}$ equal to the differences in time between these two mean phases. Now, amongst the observations at the five stations at which durations have been observed, some three differences between the two phases may be collected. Their mean value agrees so closely with that deduced from my equations that all these mixed observations might have been included without changing my result. I look upon this point as one of the utmost importance. Had the solution been a forced solution the probability of such an agreement would be small indeed.

The ten durations are all satisfied. The necessary condition is satisfied by the value of $\frac{x-y}{2}$. What reasons, therefore, have we to doubt the result? It must be remembered by M. Faye that the onus *now rests upon him* of proving, and not asserting, that some one of my observations has been erroneously referred to mean phase in such a way as to seriously affect my results; or that there are observed durations which are without doubt correct, and which are inconsistent with my solution.

Now to what extent has M. Faye been enabled even to throw doubt upon my distribution of observations? With respect to the Wardhus observations, he *thinks* that "fulmen $32^m\ 48^s$" is an observation of real contact at ingress, and he would prefer using this and Hell's observations of real contact at egress to using apparent contacts. Now the question is, are we certain that fulmen $32^m\ 48^s$ represents any attempted observation of real contact? I doubt it for the following reasons:—Professor Littrow states that "fulmen $32^m\ 48^s$" only occurs in the darker ink in which some corrections and explanatory words were afterwards added. In the fac-simile of this important part of Hell's Journal, which Professor Littrow has done me the honour to forward to me, I

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find this observation completely barred off from Hell's observations; in fact, cramped in at the top of Sajnovics' observations.

I remark also that the words employed in describing the phenomena at ingress are similar to those employed in describing the corresponding phenomena at the egress. We have a very clear description of the phenomena observed at the real contact at egress. The description is certainly very different indeed from the laconic use of a single word, *fulmen*. I believe that this most probably refers to that apparent flowing in of the light upon both sides which is so clearly described by Wales at the Hudson's Bay observations of ingress. "We took for the instant of the first internal contact the time when the least visible thread of light appeared behind the subsequent limb of *Venus*, but before that time *Venus's* limb *seemed within that of the Sun*."

I do not say that this is the true explanation of the introduction of these words, but I do say that "fulmen $32^m\ 48^s$" is not entered as an observation like the others in due order. It is completely barred off from Hell's observation, and its meaning is exceedingly doubtful. Having at this station two complete observations of apparent contacts, I rest my discussion on this sure basis in preference to the introduction of materials so doubtful as the time opposite this entry of *fulmen*.

It may perhaps be thought that this is a critical point in my argument. Such, however, is not the case. If I had used the "fulmen $32^m\ 48^s$," as an observation of real contact, my results would have been but little modified. Take it as an observation of real contact, which I do not however believe it to be, and combine with Hell's observation of real contact at egress, and we have a duration which is represented with a residual error of about 9^s *by my solution:* a discordance not large enough to alter much the resulting parallax, but large enough to justify the refusal to introduce so doubtful an observation. I preferred, and I still prefer, to use the more certain materials of the apparent contacts. With respect to the accuracy with which these apparent contacts can be observed, the discordance of Hell's and Sajnovics' observations is only $4^s{\cdot}5$.

With respect to the St. Joseph observations, I have so fully discussed this point in my answer to Mr. Newcomb, that I shall say but little here. I believe Chappe's words; and Pauly's observation of some contact at egress, 22^s before Chappe's observation, *proves* that Chappe did not observe a real contact or the *first* appearance of the connecting ligament. I believe it proves *à priori* that he really caught some phase much nearer the mean phase of apparent contacts. If however there is any doubt upon the point, appeal must be made to the residuals. Now with my interpretation all these residuals, and all other residuals, are in beautiful accord. This is admitted. On M. Faye's assumption all the residuals are in discord. I am really quite unable to understand M. Faye's inference from the fact that, if we arbitrarily change the interpretation of the egress observa-

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tions at St. Joseph from apparent to real contacts, we throw the residuals which were all before in agreement into disagreement. M. Faye infers that this proves that the agreement of all the residuals was before forced. I take it most clearly to prove that M. Faye's interpretation is not the true one. If we knew the true values of the phase correction at ingress and egress there would remain in the residuals nothing but the error of assumed parallax, which, as my result differs only by $0''.05$ from that which M. Faye wishes to be considered the truth, would amount to less than 2^s. Moreover, with M. Faye's interpretation, we have all the other residuals thrown into confusion, and particularly the Hudson's Bay observations, which are undoubtedly observations of real contacts, each saddled with a discordance of the same sign, and amounting to $7^s.9$ and $8^s.9$ respectively. We have, moreover, with M. Faye's interpretation, two values of solar parallax $8''.91$, identical with my own, from the apparent contacts, and $8''.70$, from M. Faye's attempt to improve the real contact observations. I must here, as before, recall to M. Faye's mind that, *cæteris paribus*, harmony in the results is a proof of truth. If he denies the truth, he must prove, not assume, that I have referred my contacts to a wrong mean phase, or that there are well-observed durations incompatible with my solution.

I next pass to the Otaheite observations. I have taken the apparent contacts of the "penumbra as dark, or nearly as dark, as that of the planet itself," to be apparent contacts. M. Faye admits that they are not real contacts. He thinks however, that they are some phase between real and apparent contacts. Such an impression can only be tested by the agreement or disagreement of the results. Such a test is dead against M. Faye's impression. The observations are all in agreement. But what reasons does M. Faye bring forward that render such an impression even plausible? He thinks that the form of the reflectors made use of by all the observers must have been distorted, so that the image of a point was no longer a point. The observers made no mention of any want of definition. M. Faye then assumes that such want of figure, which would undoubtedly injure definition, would seriously interfere with an observation of apparent contact when the limb of the planet and of the Sun are similarly reflected. I have not the slightest belief in the production of such an effect, to any important amount, even if the cause did exist. M. Faye must remember that I have allowed 3^s for probable error of each contact. We are not, fortunately, in this inquiry concerned with $0^s.5$ of a second. My own explanation of this penumbra is that it is nothing more than the fuzzy light which appears around the edge of a dark body projected on a bright body like the Sun. I saw a similar phenomenon at the last transit of *Mercury*. The observers in 1769 were impressed with the opportunity presented by a transit of *Venus* to detect the presence of an atmosphere; any such appearance of

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stray light about the edges of *Venus* would, if it once attracted attention, be naturally deemed of some importance. The name penumbra is by no means an unsuggestive name for such an appearance to any one who did not attribute the appearance to its true cause, irradiation.

Such are the points in which M. Faye takes exception to my work.

He thinks that I ought to have used something, we know not what, as an observation of real contact at Wardhus. Had I used this it would have but little modified my result. It may be an observation of real contact: it may be, as I have suggested, an indication of the appearance of light nearly all round the planet. I only say that it is not inconsistent with my solution in any case, but that I prefer to rest the solution upon a more intelligible basis. M. Faye prefers to use Chappe's observation at egress as a real contact. I say that Pauly's observation shows this to be inadmissible, and the residuals say the same. Chappe's own words, in my opinion, tend to the same conclusion. M. Faye thinks that the Otaheite observations of apparent contacts of penumbra are something between a real and an apparent mean phase. The residuals say what the observer's own words appear to indicate, that they are observations of apparent contact. Assume that, in all the cases specified, M. Faye's interpretations were as simple and as reasonable as my own (I am very far, indeed, however, from granting this) — what then? my assumption leads to perfect harmony amongst the observations themselves. M. Faye throws all again into chaos. M. Faye has certainly overlooked the fact that the observers did observe something. The so-called errors of observation would be reduced to nothing if we only knew the proper corrections to reduce all the observations to the same phase. With respect to the probable error of my result, I have assumed the probable error of the deviation of such observations from the corresponding mean phase to be 3^s. The probable error of the resulting parallax is then less than $0''.03$. All I can say is, that the residuals do not appear to require a larger assumption of probable error. However, if we assume 6^s, the probable error of the result would be only be about $0''.05$. The very form which all discussions are now taking upon transits of *Venus* and transits of *Mercury;* questions of irradiation; questions whether observations refer to real or apparent contacts; separation, justly enough, of observations of contacts of *Mercury* into classes; shows the flood of light which has been let into this question since the appearance of my paper. Transits of *Mercury* have taken place before: the phenomenon of the black drop was known; but it was not known that the value of the solar parallax which resulted from the discussion of the observations of the transit of 1769 was entirely a question of the reference of a few observations to apparent or real contacts. It was not known that by interpretations put upon the observer's words, at least as simple as any interpretations which can be put upon them, the whole of the observations collected could

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be reduced into perfect harmony. And that the value of the solar parallax which will *alone* reduce these observations to harmony is a value so close to what we now believe the truth, that no one method, and probably not the mean of all the other methods, can say how much that error may be. I leave my work with confidence to the judgment of astronomers, knowing that justice will be fully done, and whatever my real claims may be they will ultimately be fairly recognised.

It may perhaps be desirable that attention should be called to the agreement which holds between the results of my paper "On the Rediscussion of the Transit of Venus, 1769," and the numerous observations of ingress obtained in England, Southern Europe, and on the American Continent; also with a few scattered observations of egress. These observations were not employed in my discussion. Their introduction would not, in my opinion, have added sensibly, if at all, to the true weight of the final result. It is however of course necessary that my solution, if a true solution, should not be inconsistent with their evidence. This point had received my most serious consideration before the appearance of my paper. I considered the evidence thus afforded was confirmatory of my general solution. I would remark that the observations which I have made use of are the best for several reasons; they are free from questions of absolute time, a point of paramount importance. The observers at these stations all used, so far as we have any information on the point, considerable magnifying powers, and, what is of still more importance, powers very nearly equal. From the importance of the observations at these selected stations we may reasonably suppose that their instrumental equipments were the best available at the time. From their drawings and verbal descriptions their attention appears to have been drawn to the observations of the same two principal points. These observations were made under fair atmospherical conditions. We cannot, therefore, expect to have so good an agreement amongst miscellaneous results, collected under unfavourable circumstances, or with very different powers and complicated with errors of absolute time, as amongst the results made under especially favourable conditions and entirely free from all systematic errors except the rates of their clocks.

The observations made in England and Southern Europe were all made under most unfavourable circumstances. The Sun at Greenwich was less than 5° high at internal contact, and lower considerably in France, most of the observations having been made at a less altitude than $3\frac{1}{2}$°. Many of the observations were made with low powers; the results are all complicated by errors of absolute time. The attention of the observers appears to have been directed to very different methods of estimating the time of real contact; some even waited for the time when the disk of *Venus* appeared again perfectly round,—a very rough observation, I presume, at such low altitudes. Under such circumstances

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great discordances might be expected. The observations are, in fact, so discordant that only one method appears to me available for making any use of them. If we collect all the observations for which the factor of parallax is nearly the same, and which appear or profess to be observations of apparent contact, and take their mean; and if we proceed exactly in the same manner with the observations of real contact, the mean of the two results thus obtained should correspond with some accuracy to a phase half-way between the apparent and mean phases. This method may appear rough, but the materials are rough, and no objection can be urged against its fairness. From 89 observations thus available I have found a residual of $+ 22^s$. My method may perhaps be better understood if we confine our attention to the observations made at the Royal Observatory, Greenwich. We have here four observations of "Regular circumference of Sun and *Venus* in contact." The mean apparent time is $7^h\ 28^m\ 45^s$. We have five observations of "Completion of thread of light." The mean is $7^h\ 29^m\ 23^s$ A.T. The mean of these times, $7^h\ 29^m\ 4^s$ A.T., should correspond to the mean phase very nearly. The residual for this time for this station is $+ 25^s$, agreeing pretty closely with the general mean. I will now deduce from my general solution what should be the residual for mean phase at Greenwich, assuming the mean of Hell's and Sajnovics' observations of apparent contact at Wardhus to be free from error. We have from these observations,

$$35^s + 10{\cdot}24\, d\alpha - 16{\cdot}24\, d\Delta - 44{\cdot}5\, d\Pi - 19{\cdot}7\, \varrho = 0.$$

If A and T are the residuals for apparent contact and true contact at Greenwich,

$$A + 10{\cdot}14\, d\alpha - 16{\cdot}36\, d\Delta - 48{\cdot}4\, d\Pi - 19{\cdot}7\, \varrho\, d\Pi = 0.$$

or sensibly, since $d\Pi = 0''{\cdot}42$,

$$A = 35 + 3{\cdot}9\ d\Pi = 36{\cdot}6$$
$$T = A - 16{\cdot}6 \qquad = 20{\cdot}0.$$

Consequently

$$\frac{A + T}{2} = 28^s{\cdot}3.$$

The quantity obtained from the Greenwich Observations is 25^s.

I think this agreement is sufficient to show that the Wardhus Observations and the Greenwich Observations are reconciled by my solution. The residual for Greenwich, 25^s, is rather larger than the general mean, but this general mean is drawn down by some observations made in the South of France, where the circumstances of observation were still much more unfavourable than at Greenwich, as stated above.

I shall next pass to the consideration of the numerous observations made on the North American Continent. These observations were made under fair atmospherical circumstances, and

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their agreement is good, especially when free from uncertainties of absolute time. The instrumental equipments of some of the observers were, however, rather imperfect, and uncertainties still hang over the determinations of the longitudes of some of the stations, and even of the exact position of some of the observing stations. Here, again, I shall lay more stress in representing the mean of the observations, than in representing with great accuracy one selected observation.

The following residuals are extracted from Encke's discussion, with corrections to assumed longitudes extracted from a table in M. Powalky's discussion.

Station.	Observer.	s	
Philadelphia	Shippen	+21	real contact.
,,	Williamson	30	apparent contact.
,,	Thomson	27	
,,	Prior	29	
,,	Erving	31	

Shippen states that he tried to observe the completion of the thread of light. I shall take, therefore, Shippen's to be an attempt at a real contact, and the other observations as attempts at apparent contacts.

Station.	Observer.	s	
Norriton	Lukens	40·9	apparent contacts.
,,	Smith	37·9	

The description of the phenomena observed is not very clear, but the observations appear to me to be attempts at apparent contacts; Ritterhouse observed, at the same station, some connexion long after the times given for these contacts, which can therefore, only be assumed to be attempts at apparent contacts.

Cambridge.

Professor Winthrop, + 9ˢ·2. Real contact.

This observation is of great value. The observer's position is well known. He took great care to determine the time with accuracy.

Station.	Observer.	s	
Providence	West	+1·0	real contact?
Wilmington	Poole	+3·9	real contact.
Lewes	Biddle	−9·0	real contacts.
,,	Bailey	−5·0	
Newbury	Williams	−0·3	real contact.
Baskeridge	Stirling	−0·0	real contact.

These results would give the following residuals,—

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For apparent contacts	+32·6	mean 17ˢ·6.
real contacts	+ 2·6	

The residual for the real contact is made small by the observations at Lewes, and is, of course, affected by any error in the assumed longitude ; no observations are given to test the accuracy with which the time was determined.

Then, assuming as before the Wardhus observation to be correct, I deduce from my solution for the mean station of these observations

For apparent contacts	+28·1	mean 19ˢ·8.
real contacts	+11·5	

The mean results of calculation, based upon the Wardhus observations and my solution, and the results of the observations of these fourteen observers, agree within about 2ˢ. The residual for real contact from my solution is almost identical with the means of Shippen's, Winthrop's, and Poole's results.

If the means of these American observations are taken without any attempt to discriminate between the phases, the residuals cannot be expected to agree so closely. The difference is, however, only about 5ˢ. There is a most valuable group of observations made at St. Domingue by Pingré, Fleurieu, La Filière, and Destourès. The observations are entered as

" *Contact intérieur des bords du Soleil et de Vénus.*

Temps vrai. h ′ ″	
2 44 45	Selon M. de Fleurieu.
2 44 41	,, M. le Chevalier de la Filière.
2 44 50	,, M. Destourès.
2 44 44	,, M. Pingré."

I assume these observations to be those of apparent contact or of contact of limb with limb. Assuming, as before, the Wardhus observations as correct, I find that the mean of these results is represented within 1ˢ by my general solution. If we assume the mean of the Otaheite observations to be true, we can represent these observations within 3ˢ.

With respect to the egress observations, if we take the mean of Green's and Cook's observations to be the true time of apparent contact at Otaheite, and assume the results of my re-discussion, we can represent the observations with factor about + 42·6 $d\,\Pi$, viz. the real contact observations of Lowitz's, Inochodsow, and Kraft's at Gurief and Orenbourg within 5ˢ; the mean of Mohr's and Dollières' observations at Batavia and Pekin, with factor about + 28·4 $d\,\Pi$ within about 4ˢ; the apparent contact of Collas, at Pekin, within about 2ˢ. These are really all the checks which we can apply to the Wardhus and Otaheite

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observations respectively which are not applied in my re-discussion, and these checks appear to me all confirmatory of the interpretations I have put upon the Wardhus and Otaheite observations.

The only point which does not appear to me perfectly confirmatory of my solution is the apparently rather close agreement of the Hudson Bay observations of ingress, which are undoubtedly real contact observations, with the apparent contacts at Wardhus. Upon this I can only remark that the longitude of this station depends upon the discussion of a few occultations made nearly one hundred years ago; that this difficulty arises entirely from the great difference in the longitude deduced from these same occultations by M. Powalky from that previously deduced by Encke; that M. Powalky's longitude makes the egress observations entirely disagree with other undoubted observations of real contact; *that the durations at this station are perfectly represented in my solution.* The uncertainties still resting on the longitudes of this station and of St. Joseph afford the best justification of the method I adopted in making the discussion depend solely upon durations. I am more than ever convinced of the extreme importance of observing the so-called apparent contacts as well as the so-called real contacts. It will be seen from what precedes that a mean between apparent and real contacts is a phase represented with accuracy. In conclusion, I may, perhaps, be allowed to suggest that discussions which reject the observations at *Otaheite or Wardhus and Kola* as inconsistent with their results are not of very much weight. The strain on the truth of the result is in these cases really slipped, not borne.

My object in this addition is simply to afford independent proofs that the adopted times for Wardhus and Otaheite are not much in error; if these observations are sensibly correct, there can be no doubt about the value of solar parallax.

Addition to Mr. Proctor's Paper on the Transit of Venus in 1874. (*Monthly Notices for March 12th*, 1869.)

(*Communicated by Mr. Proctor.*)

The four following corrections refer to printer's errors:—

Page 213, in the expression for V, $d'^{\frac{3}{2}}$ *should be* $d'^{\frac{1}{2}}$.

Page 215, line 14 from bottom, *for* elliptical *read* ecliptical.

Page 217, line 9 from top, *for* l' *read* λ'.

Page 217, line 8 from bottom, *for* 54° 27′ *read* 44° 27′.

The last correction is important, though the context (*see* p. 218, line 12) sufficiently shows what was intended.

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I have gone very carefully through the calculations on which the results in my paper were founded, and have detected a slight error affecting the last three longitudes in the table on p. 217. The error (one of transcription) does not invalidate, but on the contrary rather strengthens, the conclusions arrived at in my paper. As the first of the three longitudes just named was also wrongly printed, it seems best to give the whole table, as corrected :—

	Lat.	Long.
(i.) The place at which first internal contact is most accelerated lies in	39 45 N.	143 23 W.
(ii.) The place at which first internal contact is most retarded lies in	44 27 S.	26 27 E.
(iii.) The place at which last internal contact is most accelerated lies in	64 47 S.	114 37 W.
(iv.) The place at which last internal contact is most retarded lies in	62 5 N.	48 22 E.

I have calculated the positions of the corresponding places with the elements given by Mr. Airy in the *Monthly Notices* for December 11th, 1868, p. 34. These elements are not strictly congruous, it will be noticed, but I use them without change; except that I have ventured to alter the value of the Sun's declination, which is obviously incorrect. The places corresponding to (i.) and (ii.) lie respectively in north and south latitude 37° 13′ and in longitude 138° 38′ W. and 41° 22′ E., respectively. The places corresponding to (iii.) and (iv.) lie respectively in north and south latitude 60° 1′, and in longitude 136° 11′ W. and 43° 49′ E., respectively. The distances of these four places from those tabulated above I find to be (by the method of mid-latitudes) 314·0, 920·2, 764·5, and 208·7 miles, respectively.

On some Effects of the Comparative Clinging of the Limb of Venus to that of the Sun in the Transit of 1874, *as compared with that of* 1882. By E. J. Stone, Esq.

There is one point of great practical importance which must not be overlooked in estimating the relative values of the Transits of *Venus*, 1874 and 1882, and in determining which of the methods proposed for utilizing the Transit of 1874 is the more valuable. The chord described in 1874 is considerably less than the corresponding chord in 1882. M. Puiseux has shown, however, that very considerable differences of duration can be obtained in the Transit of 1874, and his results have been confirmed by Mr. Proctor. The same cause, however, which renders this possible renders the observations themselves less accurate. The object of this short Note is to obtain an estimation of the injurious effects

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upon the observations themselves arising from the slow motion of *Venus* across the Sun's limbs, and to trace the consequences upon the methods proposed by the Astronomer Royal and M. Puiseux for utilizing the Transit of 1874. The difficulty in deducing a value of Solar Parallax from observations of contacts of *Venus* and the Sun is that of referring the observations to the same phase. We are really, therefore, concerned with estimations of angular measures by their equivalent times. I find from Mr. Hind's calculation, *Comptes Rendus*, 1861, July 22, that, with the same assumed semidiameter of *Venus*, the time from external contact to internal contact in the Transit of 1874 is no less than 29^m, whilst in that of 1882 it is only $20^m\ 19^s$. From this I conclude that, if e be the probable error in time of an observation in 1882, the corresponding probable error for the Transit of 1874 will be

$$e \times \frac{29^m}{20^m\ 19^s} = e\,\frac{1740}{1219}.$$

A difference of durations of $20^m\ 19^s$ is, therefore, in 1882 as valuable as one of 29^m in 1874. Other differences are effective in like proportion. It appears to me impossible to secure good observations in 1874 with such differences as those mentioned by Mr. Proctor. It must be remembered that it is useless to discuss differences obtained from the consideration of stations at which the necessary observations cannot be made on account of the small altitudes of the Sun at the times of internal contacts. For my part I should at once exclude from consideration all stations with altitudes of the Sun, at either ingress or egress, much less than 10°. I consider even this too low, unless a good deal has to be sacrificed in parallax-factor to secure greater elevation.

How small the value of observations made within 5° of the horizon is, will at once appear by a comparison of the English and French observations of the Transit of 1769 with other observations made at greater altitudes of the Sun.

The relative effects of the increased probable error of internal contacts in 1874, on the method proposed by the Astronomer Royal and M. Puiseux, is seen from the following formulæ:—

Let l be the probable error of a longitude determination,
e that of an internal contact,
then (constant)^2 (probable error of parallax)^2

$= 4e^2$. In the method of durations.
$= 2(e^2 + l^2)$. In Mr. Airy's method.

the relative weights are therefore

$$\frac{\text{Puiseux}}{\text{Airy}} = \frac{(\text{Puiseux constant})^2}{(\text{Airy constant})^2} \cdot \frac{1}{2}\left\{1 + \frac{l^2}{e^2}\right\}$$

Since, therefore, l is constant for all times, and e is greatly

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increased in 1874, we see at once from the formula that the method of durations contrasts much more unfavourably with that of absolute time determination in 1874, than in that of 1882 or in that of 1769. This is probably the ground upon which the Astronomer Royal still gives his preference to absolute time determinations for the Transit of 1874. The formula can very approximately be turned into numbers. The Astronomer Royal considers that we are now in a position to determine longitudes with a probable error of 1^s. In my re-discussion of the Transit of *Venus*, 1769, I have assumed 3^s for the probable error of an internal contact. This appears to cover the residuals, but it has been considered too small by some astronomers. Taking it, however, at 3^s for 1882, we must assume $e = 4^s{\cdot}28$ for 1874. In order then that the probable errors of M. Puiseux's method and that of Mr. Airy should be equal, we must have differences of duration of 31^m in M. Puiseux's method to correspond to differences of 22^m in Mr. Airy's. This of course depends upon the possibility of determining longitudes within 1^s. I do not think that the probable error of internal contacts has been over-estimated.

The method proposed by the Astronomer Royal possesses also another advantage; it removes the necessity of attempting to push observers into the Antarctic regions. Kerguelen's Islands and the Macdonald Islands are, however, favourable positions for both methods, and there appears every reason to avail ourselves of both methods to at least this extent. From the slow motion of *Venus* from the Sun's limb, I do not consider the Transit of 1874 a favourable one.

Blackheath,
1869, *April* 8.

Occultations of Stars by the Moon and Phenomena of Jupiter's Satellites, observed at the Royal Observatory, Greenwich, from 1868, *January, to* 1869, *March.* Communicated by G. B. Airy, Esq., Astronomer Royal.

Occultation of Stars by the Moon.

Day of Obs. 1868.		Phenomenon.	Moon's Limb.	Mean Solar Time. h m s	Observer.
Feb. 28	(*a*)	μ Ceti, disappearance	Dark	9 24 45·7	J.C.
		μ Ceti, reappearance	Bright	9 47 51·1	J.C.
Mar. 1		θ^2 Tauri, disappearance	Dark	6 38 24·5	E.
		θ^1 Tauri, disappearance	Dark	6 38 37·0	E.
May 4	(*b*)	l^2 Virginis, disappearance	Dark	9 7 35·6	C.
	(*c*)	l^2 Virginis, reappearance	Bright	10 21 36·3	C.

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Day of Obs. 1868.		Phenomenon.	Moon's Limb.	Mean Solar Time. h m s	Observer.
May 27		18 Leonis, disappearance	Dark	10 30 59·3	D.
Dec. 27	(*d*)	B.A.C. 1526, disappearance	Dark	6 27 51·2	J.C.
1869.					
Jan. 24		119 Tauri, disappearance	Dark	8 14 33·3	D.
		120 Tauri, disappearance	Dark	8 47 42·5	D.

(*a*) Cloudy.
(*b*) The disappearance did not seem to take place so suddenly as usual.
(*c*) Thought to be a little late.
(*d*) Uncertain to a second; clock beats scarcely audible from high wind.

Phenomena of Jupiter's Satellites.

Day of Obs. 1868.	Satellites.		Phenomenon.	Mean Solar Time. h m s	Observer.
Jan. 15	III.		Occult. reappearance, first contact	6 4 15·6	G.K.
			,, ,, bisection	6 5 34·4	G.K.
			,, ,, last contact	6 6 5·3	G.K.
			Eclipse, disappearance	6 16 53·5	G.K.
Feb. 1	I.	(*a*)	Eclipse, reappearance	5 44 2·5	J.C.
July 13	II.	(*b*)	Eclipse, reappearance	12 14 28·6	J.C.
	II.	(*c*)	Occult. disappearance, bisection	12 25 16·9	J.C.
19	III.		Eclipse, disappearance	14 52 26·2	H.C.
23	III.	(*d*)	Transit, egress, last contact	13 14 29·8	C.
Aug. 24	I.		Eclipse, disappearance	10 42 4·6	C.
Sept. 4	III.	(*e*)	Transit, egress, bisection	10 35 40·6	H.C.
	III.		,, ,, last contact	10 40 24·8	H.C.
10	I.		Transit, egress, first contact	9 1 31·9	L.
	I.		,, ,, bisection	9 3 1·6	L.
	I.		,, ,, last contact	9 5 1·3	L.
24	I.		Transit, ingress, last contact	10 18 43·4	D.
	I.		Transit, egress, bisection	12 27 52·5	D.
	I.		,, ,, last contact	12 31 22·0	D.
29	III.	(*f*)	Occult. reappearance, last contact	9 59 15·1	C.
Oct. 1	II.		Transit, egress, bisection	10 58 48·6	J.C.
11	I.		Eclipse, reappearance	7 48 7·0	D.
16	I.		Occult. disappearance, first contact	12 37 12·8	C.
	I.		,, ,, bisection	12 39 27·4	C.
17	II.		Occult. disappearance, bisection	7 48 19·6	D.
	II.		,, ,, last contact	7 50 19·3	D.
24	III.		Transit, egress, last contact	9 49 33·3	E.
	II.		Occult. disappearance, first contact	10 0 31·5	E.
Nov. 2	I.		Transit, ingress, bisection	7 52 47·3	C.
	II.		Transit, egress, last contact	9 20 3·1	C.
	I.		,, ,, first contact	10 0 26·5	C.
	I.		,, ,, last contact	10 3 55·9	C.

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Days of Obs. 1868.	Satellites.		Phenomenon.	Solar Solar Time. h m s	Observer.
Nov. 18	III.		Eclipse, disappearance	11 27 43·2	E.
Dec. 3	I.		Eclipse, reappearance	10 15 33·5	D.
11	I.	(*g*)	Transit, egress, last contact	8 9 1·4	C.
	II.	(*h*)	Transit, ingress, last contact	8 26 28·6	C.
12	I.		Eclipse, reappearance	6 40 56·0	E.
19	I.		Eclipse, reappearance	8 36 21·3	J.C.
31	III.		Occult. disappearance, bisection	5 59 37·8	J.C.
	III.		Occult. reappearance, last contact	8 58 8·9	J.C.
1869. Jan. 4	I.		Eclipse, reappearance	6 57 46·0	S.
5	II.		Transit, egress, last contact	8 7 16·7	C.
Feb. 5	III.		Eclipse, disappearance	7 59 4·4	E.

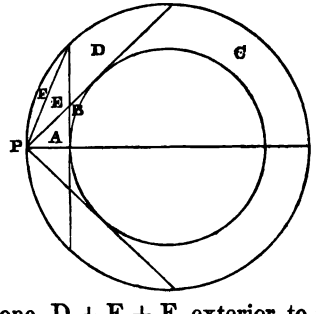
(*a*) (*d*) The image very bad.
(*b*) Uncertain, the planet being badly defined, and the satellite reappearing very near it.
(*c*) The planet very low and the obervation uncertain.
(*e*) The image very tremulous; the observation uncertain.
(*f*) Doubtful, in consequence of the planet being obscured by cloud every few moments.
(*g*) Very uncertain; the image of the planet very tremulous; clouds passing.
(*h*) Very tremulous.

The initials S., D., E., C., L., J.C., H.C., and G.K., are those of Mr. Stone, Mr. Dunkin, Mr. Ellis, Mr. Criswick, M., Lynn, Mr. Carpenter, Mr. H. Carpenter, and Mr. Keating.

Note on the Attraction of Ellipsoids.

By Prof. Cayley.

If an indefinitely thin shell of uniform density, bounded by two similar and similarly-situated ellipsoids, attracts a point P on its outer surface, it has been shown geometrically by M. Chasles that the attraction is in the direction of the normal at P, and is equal to twice the attraction of an infinite plate, the thickness of which is equal to the normal thickness at P. Assuming that the attraction is in the direction of the normal, the proof is in fact as follows:—with P as vertex, circumscribe to the interior surface a cone; this divides the shell into three parts; the one, D + E + F, exterior to the cone, the other two, A + B and C, interior to the cone. It is shown that in the direction of the



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normal the attraction of C is equal to that of A + B; and it is assumed that in comparison with these the attraction of D + E + F may be neglected; the whole attraction is thus equal to twice that of the portion A + B. At the point where the normal at P meets the internal surface draw the tangent plane to the internal surface, thus dividing the portion A + B into the solid cone A and a remaining portion B; it is assumed that in comparison with that of A the attraction of B may be neglected; the whole attraction is thus equal to twice the attraction of the solid cone A; and the attraction of this solid cone is in the limit (the aperture or solid angle then becoming $= 2\pi$) equal to the attraction of an infinite plate whose thickness is equal to the altitude of the solid cone, that is, to the normal thickness at P. And the attraction of the whole ellipsoidal shell is thus ultimately (that is, when the shell is indefinitely thin) equal to twice the attraction of the infinite plate.

It is interesting to ascertain the orders of magnitude of the attractions of the several portions of the shell, which attractions are compared in the foregoing investigation; and this can be done very easily, when, instead of the ellipsoidal shell, we have a spherical shell (bounded by two concentric spherical surfaces). The tangent plane to the inner surface divides the portion D + E + F into two portions D and E + F; and if with P as vertex we describe a cone standing on the circle in which the tangent plane meets the outer surface, the last-mentioned portion is hereby divided into the portions E and F; the whole shell is thus divided into the portions A, B, C, D, E, F, each of them symmetrical in regard to the normal or radius at P, and consequently attracting in the direction of this radius. I proceed to find the attractions of each of these portions; it will appear, in accordance with the assumptions of the foregoing investigation, that, taking the radii to be 1 and $1 + \alpha$, that is, α the thickness of the shell, and supposing ultimately α to become indefinitely small, the attractions of A and C are each ultimately $= 2\pi\alpha$, that is $=$ to the attraction of the infinite plate, while the attractions of the other portions are of the order $\alpha^{\frac{3}{2}}$, and thus vanish in comparison with that of A or C.

The attraction of an indefinitely thin cone or frustum of a cone, length r and solid angle $d\omega$ is $= r\,d\omega$; considering any such cone having P for its vertex, if the inclination of r to the radius through P is $= \theta$, and if the azimuth of the plane through r and the radius is $= \phi$, then we have $d\omega = \sin\theta\,d\theta\,d\phi$, the attraction $r\,d\omega$ is $= r\sin\theta\,d\theta\,d\phi$, and this attraction resolved in the direction of the radius is $= r\sin\theta\cos\theta\,d\theta\,d\phi$. For the several cases which have to be considered, the value of r is independent of ϕ, and the integration in regard to ϕ is always from $\phi = 0$ to $\phi = 2\pi$;—the attraction is thus in each case $= 2\pi\int r\sin\theta\cos\theta\,d\theta$, the expression of r in the terms of θ, and the limits of θ being known for each of the several portions of

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the shell. Taking θ_1 for the semi-angle of the tangent cone, we have it clear

$$\sin\theta_1 = \frac{1}{1+\alpha},\quad \cos\theta_1 = \frac{\sqrt{2\alpha+\alpha^2}}{1+\alpha};$$

and taking θ_2 for the semi-angle of the cone which divides the portions E, F,

$$\tan\theta_2 = \sqrt{\frac{2+\alpha}{\alpha}},\quad \sin\theta_2 = \frac{\sqrt{2+\alpha}}{\sqrt{2(1+\alpha)}},\quad \cos\theta_2 = \frac{\sqrt{\alpha}}{\sqrt{2(1+\alpha)}}.$$

For F we have

$$r = 2(1+\alpha)\cos\theta,\quad \theta = \theta_2 \text{ to } \theta = \frac{\pi}{2},$$

Integral is

$$= 2(1+\alpha)\int \sin\theta\cos^2\theta\, d\theta = \frac{2}{3}(1+\alpha)\cos^3\theta_2.$$

For D + E we have

$$r = 2(1+\alpha)\cos\theta,\quad \theta = \theta_1 \text{ to } \theta = \theta_2,$$

Integral is

$$= 2(1+\alpha)\int \sin\theta\cos^2\theta\, d\theta = \frac{2}{3}(1+\alpha)_3(\cos^3\theta_1 - \cos^3\theta_2).$$

For E, we have

$$r = \frac{\alpha}{\cos\theta},\quad \theta = \theta_1 \text{ to } \theta = \theta_2,$$

Integral is

$$= \alpha\int \sin\theta\, d\theta = \alpha(\cos\theta_1 - \cos\theta_2).$$

For A we have

$$r = \frac{\alpha}{\cos\theta},\quad \theta = 0 \text{ to } \theta = \theta_1,$$

Integral is

$$= \alpha\int \sin\theta\, d\theta = \alpha(1-\cos\theta_1).$$

For A+B we have

$$r = (1+\alpha)\cos\theta - \sqrt{1-(1+\alpha)^2\sin^2\theta},\quad \theta = 0 \text{ to } \theta = \theta_1,$$

Integral is

$$= \int \left\{(1+\alpha)\cos\theta - \sqrt{1-(1+\alpha)^2\sin^2\theta}\right\}\sin\theta\cos\theta\, d\theta$$

$$= (1+\alpha)\left(-\frac{1}{3}\cos^3\theta\right) + \frac{1}{3(1+\alpha)^2}\left\{1-(1+\alpha)^2\sin^2\theta\right\}^{\frac{3}{2}}, \text{ between the limits.}$$

$$= \frac{1}{3}\left\{(1+\alpha)(1-\cos^3\theta_1) - \frac{1}{(1+\alpha)^2}\right\}.$$

and subtracting the above value of the integral for A, it at once appears that, for B, the integral is

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$$= 2\pi \left\{ \alpha (-1 + \cos\theta_1) + \frac{1}{3}(1+\alpha)(1-\cos^3\theta_1) - \frac{1}{3}\frac{1}{(1+\alpha)^2} \right\}.$$

Hence, calculating the approximate values, and restoring in each case the omitted factor, 2π, we have

$$\text{Attraction } A = 2\pi\alpha - 2\sqrt{2}\,\pi\alpha^{\frac{1}{2}},$$

$$,, \quad B = \frac{2}{3}\sqrt{2}\,\pi\alpha^{\frac{1}{2}},$$

$$,, \quad C = 2\pi\alpha - \frac{4}{3}\sqrt{2}\,\pi\alpha^{\frac{1}{2}},$$

$$,, \quad D = \frac{4}{3}\sqrt{2}\,\pi\alpha^{\frac{1}{2}},$$

$$,, \quad E = 1\sqrt{2}\,\pi\alpha^{\frac{1}{2}},$$

$$,, \quad F = \frac{1}{3}\sqrt{2}\,\pi\alpha^{\frac{1}{2}}.$$

Or, if we please,

$$\text{Attraction } A+B = 2\pi\alpha - \frac{4}{3}\sqrt{2}\,\pi\alpha^{\frac{1}{2}},$$

$$,, \quad C = 2\pi\alpha - \frac{4}{3}\sqrt{2}\,\pi\alpha^{\frac{1}{2}},$$

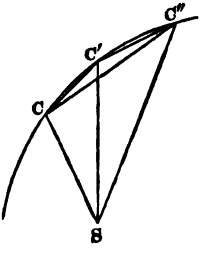
$$,, \quad D+E+F = \frac{8}{3}\sqrt{2}\,\pi\alpha^{\frac{1}{2}};$$

so that ultimately the attraction of the portion $D+E+F$ vanishes in comparison with those of the portions $A+B$ and C; and the attraction of these last, that is, of the whole shell, is $= 4\pi\alpha$, twice the attraction of an infinite plate of the thickness α.

Note on the Problem of the Determination of a Planet's Orbit from three Observations. By Prof. Cayley.

The principle of the solution given in the *Theoria Motus* may be explained very simply as follows:—

Consider three successive positions of C, C′, C″, of a planet revolving about the focus S; let n, n', n'', denote the doubles of the triangular areas C′SC″, CSC″, and CSC″ respectively (viz. the triangular area means the area of the triangle included between the two radius vectors and the chord joining their extremities), r' the radius vector SC′; θ'', θ, the times of describing the arcs CC′ and C′C″ respectively, the units of time and distance being such that the time



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is equal to the double area divided by the square root of the half latus rectum ($t = 2\pi a^{\frac{3}{2}}$ for the Period in a circular or elliptic orbit).

Then writing,

$$P = \frac{n''}{n}, \quad Q = 2\left(\frac{n + n''}{n'} - 1\right) r'^3,$$

(observe that $n + n'' - n'$ is = twice the triangle C C′ C″), for neighbouring positions of the planet, the values of P and Q are *approximately* $= \frac{\theta''}{\theta}$ and $\theta\,\theta''$ respectively: the solution consists in the determination of an orbit for which P and Q have these approximate values; then, by means of such approximate orbit, the values of P and Q are more accurately determined, and by means of these new values of P and Q, a new determination is effected of the orbit: and so on, to the requisite accuracy of approximation.

The foregoing approximate values of P and Q respectively are deduced from the accurate values

$$P = \frac{\theta''\eta}{\theta\eta''}, \quad Q = \frac{\theta\theta''}{\eta\eta''}\frac{r'^2}{r\,r''}\frac{1}{\cos f \cos f'' \cos f'};$$

where r, r', r'', are the radius vectors S C, S C′, S C″; $2f, 2f', 2f''$, are the angular distances C′ S C″, C S C″, C S C′ ($f = f' + f''$) and η, η', η'', are the ratios of the sectorial areas C′ S C″, C S C″, C′ S C′, to the triangular areas represented by the same letters respectively: the doubles of the sectorial areas are thus $n\eta, n'\eta'$, and $n''\eta''$, and if the half latus rectum be denoted by p, then we have

$$\sqrt{p} = \frac{n\eta}{\theta} = \frac{n'\eta'}{\theta'} = \frac{n''\eta''}{\theta'};$$

and it thus at once appears that the accurate value of P is $= \frac{\theta''\eta}{\theta\eta''}$, as above. To obtain the expression for Q, taking $\varphi, \varphi', \varphi''$, for the true anomalies (and, for greater symmetry, writing for the moment $\nu, -\nu', \nu'', g, -g', g''$, in place of n, n', n'', f, f', f'', respectively), we have

$$r = \frac{p}{1 + e\cos\varphi}, \quad 2g = \varphi'' - \varphi',$$

$$r' = \frac{p}{1 + e\cos\varphi'}, \quad 2g' = \varphi - \varphi'',$$

$$r'' = \frac{p}{1 + e\cos\varphi''}, \quad 2g'' = \varphi' - \varphi,$$

$$(g + g' + g'' = 0);$$

whence identically,

$$\frac{\sin 2g}{r} + \frac{\sin 2g'}{r'} + \frac{\sin 2g''}{r''} = -\frac{4\sin g \sin g' \sin g''}{p},$$

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or writing

$$\nu = r'r'' \sin 2g, \quad \nu' = r''r \sin 2g', \quad \nu'' = rr' \sin 2g''$$

this is

$$\nu + \nu' + \nu'' = -\frac{4\,rr'r'' \sin g \sin g' \sin g''}{p},$$

$$= -\frac{(rr'r'')^2 \sin 2g \sin 2g' \sin 2g''}{2\,p\,rr'r'' \cos g \cos g' \cos g''}$$

$$= -\frac{\nu\nu'\nu''}{2\,p\,rr'r'' \cos g \cos g' \cos g''}.$$

This is, in fact,

$$n - n' + n'' = \frac{n\,n'\,n''}{2\,p\,rr'r'' \cos f \cos f' \cos f''},$$

or since

$$\frac{nn''}{p} = \frac{\theta\theta''}{\eta\eta''},$$

it is

$$2\left(\frac{n+n''}{n'} - 1\right) = \frac{\theta\theta'}{\eta\eta''rr'r'' \cos f \cos f' \cos f''},$$

viz. multiplying by r'^3, it is

$$Q = \frac{\theta\theta''}{\eta\eta''}\frac{r'^2}{rr''}\frac{1}{\cos f \cos f' \cos f''},$$

the above-mentioned value of Q.

On Personality in Observing Transits of the Limbs of the Moon. By E. Dunkin, Esq.

One result of the extreme accuracy of modern astronomical observations and reductions has been the detection of numerous well-marked personal differences, or discordances, which had hitherto escaped notice. One of the first discovered of these personalities, and perhaps the most constant of them all, is that existing in the determination of the absolute time of a transit of a star across the wires in the field of view of the telescope, an operation in which observers have been known to differ more than 1^s. Well-defined personal differences were also noticed by the late Mr. Sheepshanks in the micrometrical comparison-observations of the standard yard; similar personalities have been detected in the microscope-readings of the cross-wire bisection of a division of a graduated circle; and, lately, Mr. Stone has

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pointed out a very small, but real, personal difference in the separate determinations of the error of collimation of the telescope of the Greenwich Transit-Circle, although the amount is too insignificant to affect the observations.

The personality of which the present paper treats, is one which for many years has been known to exist. For some time, however, its effect upon the observations was not investigated. So long ago as 1848, the year after the erection of the Altazimuth at the Royal Observatory, a very large personal discordance was found between the tabular errors in R.A. of the Moon observed with the Altazimuth by Mr. H. Breen and myself, by whom all the observations had, up to that time, been made with that instrument. As this comparison of results, deduced from the observations of different limbs of the Moon, is the first that I am aware of, in which the amount of personal discordance in observing the Moon's limbs, has been clearly and systematically ascertained ; I cannot do better than quote in this place the statement of the Astronomer Royal on this (as it appeared to him) singular instance of observational personality.

"The circumstances under which the large errors given by Mr. Breen occurred were so various, in respect of Moon's age and Moon's position in her orbit ; and the intermixture of the observations had been so complete, that there was no doubt whatever that this was the result of a difference in the mode of observation. And this was not a result of personal equation usually so called : for it was known from the investigations of personal equation, as exhibited in the clock-errors given by stars (which are confirmed by similar investigations made to the end of 1849), that the personal equation was small. Neither was it a different estimation of the Moon's diameter, for the difference of errors of Moon's R.A. is nearly the same, and in the same direction, whether the first limb or the second limb be observed. It is strictly speaking, a *difference between the personal equation for the Moon and that for the stars:* or it may be thus stated, that *the duration of the impression on the nerves of the eye, or the time occupied in bringing into comparison the impressions on the eye and on the ear, is not the same when the Moon is observed with the eye as when a star is observed with the eye.* The evidence of this is given by the following numbers.

"By observations of the first limb of the Moon from 1847, May 16, to 1848, May 28 :

	s
The mean of 45 errors of Moon's Tabular R.A., by Mr. Dunkin is ..	+0·53
The mean of 35 errors by Mr. Breen is	+0·99
Excess of Mr. Breen's	+0·46

"By observations of the second limb of the Moon through the same period :

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The mean of 33 errors, by Mr. Dunkin, is	+0·50
The mean of 27 errors, by Mr. Breen, is..	+0·80
Excess of Mr. Breen's	+0·30

(*Introduction, Greenwich Observations*, 1848, p. lxx.)

A similar comparison of the errors of the Moon's tabular N.P.D. gave no sensible difference between the two observers. The Astronomer Royal fully explains the probable reason of this in the same *Introduction*.

A subsequent comparison of my observations of the Moon made on the meridian with the Transit-instrument with those of the oldest and most experienced transit-observer then at Greenwich, resulted in showing that no difference existed between us; consequently the discordance was thrown entirely on Mr. Breen's observations, to which a considerable correction was afterwards applied.

As the use of Hansen's Lunar Tables was first adopted in the *Nautical Almanac* for 1862, I had intended to make the present investigation include the seven years from 1862 to 1868. Owing, however, to a protracted illness of one of the assistants in 1862; to the absence of two assistants in the island of Valencia for a considerable period; and to the employment of another in more than the usual amount of equatorial work, the four principal observers were very unequally distributed during that year. For this reason, I have used the observations only for the six years from 1863 to 1868.

The method I have employed to exhibit the personal differences is a very simple one. My object has been to determine the yearly means, for each of the four observers, of all the tabular errors in R.A. of the Moon observed with the Transit-Circle and Altazimuth, separating those of each instrument into two groups, one consisting of all the observations before opposition, and the other containing all after opposition. As the intermixture of the observations is satisfactory in each year, particularly with the Altazimuth, the mean tabular errors become comparable for the four observers, Mr. Dunkin (D), Mr. Ellis (E), Mr. Criswick (C), and Mr. Carpenter (J. C.). To satisfy myself on this point I have, for the year 1868, arranged all the errors for each observer according to the Moon's age; the mean age agrees sensibly for all, both before and after opposition. Several observations with both instruments made by other observers I have omitted solely on the ground of the paucity of the individual numbers.

The following Tables need no special explanation, further than to state that they are only intended to give the numerical results from the data as published in the columns of the *Greenwich Observations*. That the personal discordances which they exhibit are real, there can be no manner of doubt; they are probably due principally to the different values of diameter given by each observer's observations, but how much of the personality is pre-

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cisely belonging to the observations of the first or second limb is a question very difficult to answer.

The transits of the first and second limbs of the Moon, and with the Altazimuth also of the upper and lower limbs, over the systems of wires in the field of view of the telescope, are sensibly very different operations, the Moon in its progress in one case approaching the wire, and in the second case receding from it. Every person who has had any experience in this kind of observation cannot have failed to notice that the attention of the observer's mind is here occupied in performing two different modes of estimation. From this cause it is probable that the observations of the two limbs are not made under analogous circumstances, and therefore a different amount of personality may exist in each, or, in other words, the methods of two observers may be similar in the observation of one limb, and be more or less discordant in another.

Altazimuth.

In the formation of Table I. I have extracted all the tabular errors in R.A. of the Moon as determined from the Altazimuth observations. These errors have been classified into groups according to the initial of the observer's name, keeping distinct those resulting from observations made before and after opposition. The yearly means of each group are contained in the Table.

Table I.

Before Opposition.

Year.	Mean Error of Tabular R.A. by D.	E.	C.	J.C.
	s	s	s	s
1863	−0·156	−0·296	−0·139	−0·388
1864	−0·030	−0·173	−0·093	−0·363
1865	+0·005	−0·155	+0·007	−0·253
1866	+0·071	−0·059	0·000	−0·162
1867	+0·213	−0·014	+0·106	−0·057
1868	+0·162	+0·059	+0·104	−0·035
Mean	+0·044	−0·106	−0·003	−0·210

After Opposition.

Year.	Mean Error of Tabular R.A. by D.	E.	C.	J.C.
	s	s	s	s
1863	−0·073	−0·136	−0·125	−0·122
1864	+0·016	+0·014	+0·016	−0·076
1865	+0·110	+0·014	+0·041	+0·064
1866	+0·078	+0·044	+0·041	+0·119
1867	+0·241	+0·047	+0·236	+0·184
1868	+0·242	+0·176	+0·235	+0·256
Mean	+0·102	+0·027	+0·074	+0·071

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By taking the differences between the corresponding mean yearly tabular errors the numbers in the following Table are obtained. If no personality existed, and if the adopted semi-diameter used in the reductions were strictly correct, these differences would have been insensible for each observer, provided the mean tabular errors are assumed to be constant throughout the mean lunation.

Table II.

Difference between the Mean Tabular Errors of R.A. of Moon determined before and after Opposition.

Year.	D.	E.	C.	J.C.
	s	s	s	s
1863	+0·083	+0·160	+0·014	+0·266
1864	+0·046	+0·187	+0·109	+0·287
1865	+0·105	+0·169	+0·034	+0·317
1866	+0·007	+0·103	+0·041	+0·281
1867	+0·028	+0·061	+0·130	+0·241
1868	+0·080	+0·117	+0·131	+0·291
Mean	+0·058	+0·133	+0·077	+0·281

A cursory examination of the numbers in the two preceding tables will show plainly that a well-marked regular personality of some kind is exhibited in each year, without exception. It appears that all the observers give a sensibly smaller error of tabular Right Ascension in the first half of the lunation than in the second half, varying from $+0^{s}\cdot06$ from my observations to $+0^{s}\cdot28$ from those of Mr. Carpenter, the difference being equivalent to $+0^{s}\cdot22$, or $3''\cdot30$. If we were to assume that these personal differences arise chiefly from different methods of estimation of the true position of the Moon's limb, and that a similar amount of personality exists in the observations of the upper and lower limbs; then the numbers in the above table would seem to show that the semi-diameter of the Moon determined by Mr. Carpenter differs from that resulting from my observations by about $1''\cdot50$. The following table is computed from Table I., by adopting for convenience the usual habit of referring all the observer's means to mine, which for the purpose we will suppose to equal 0. The numbers in the table are therefore differences of my mean results for each year from those of the other observers

Table III.

Before Opposition.

Year.	D—E.	D—C.	D—J.C.
	s	s	s
1863	+0·140	−0·017	+0·232
1864	+0·143	+0·063	+0·333
1865	+0·160	−0·002	+0·258
1866	+0·130	+0·071	+0·233
1867	+0·227	+0·107	+0·270
1868	+0·103	+0·058	+0·197
Mean	+0·151	+0·047	+0·254

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After Opposition.

Year.	D—E.	D—C.	D—J. C.
	s	s	s
1863	+0·063	+0·052	+0·049
1864	+0·002	0·000	+0·092
1865	+0·096	+0·069	+0·046
1866	+0·034	+0·037	−0·041
1867	+0·194	+0·005	+0·057
1868	+0·066	+0·007	−0·014
Mean	+0·076	+0·028	+0·032

Referring to the preceding table it may be remarked that, before opposition, Mr. Ellis's mean tabular error agrees nearly with the mean of all, or is only slightly in excess of it. Mr. Criswick's mean tabular error is between the mean and mine, those of Mr. Carpenter and myself being at opposite ends of the scale. After opposition, excepting the comparison with Mr. Ellis in 1867, which from some unknown accidental cause stands out from the other years, no difference in the table reaches $0^s{\cdot}1$, while in the majority of instances the amount is below $0^s{\cdot}05$.

Although the major part, if not all, of the personality apparently belongs to the first half of the lunation when the first limb of the Moon has been observed, yet it is very probable, and even certain, that some part of it arises from other causes than by the different methods of observing the transits of the first and second limbs. In the reductions of the Altazimuth observations, the error of tabular R.A. is not directly obtained from the simple transit of the limb, as with the Transit-circle, but from a comparatively complicated series of observations and calculations, depending not only upon transits of the right or left limb, but also on transits of the upper or lower limb, in which personality creeps in as much probably as in the azimuthal transits, and upon micrometrical readings of the divisions of the horizontal and vertical circles. In vertical transits, the limb observed depends upon the position of the Moon with respect to the meridian and horizon at the time of observation. I find from a special examination of the printed volumes of the *Greenwich Observations*, that in the first half of the lunation the observations of the upper and lower limbs are nearly equally divided, and that nearly all those of the lower limb are observed west of the meridian, and nearly all those of the upper limb east of the meridian. The result of this is that, if the adopted semi-diameter employed in the reductions is too small or too great, the transits of the upper and lower limbs made on opposite sides of the meridian, would affect the resulting tabular error of R.A. in the same direction, and would tend to produce, if the semi-diameter be too great, a large negative error in the first half of the lunation. After opposition, the lower limb of the Moon, with very few exceptions, is always observed east of the meridian. These complications make the discovery of the true cause of the personality being

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apparently more in one limb than another a very difficult matter indeed.

If we may suppose that the principal cause is a question of personal semi-diameter, then it would not be difficult to apply separate corrections to make the errors for both azimuthal limbs alike, but there would still be an outstanding personality left in the mean absolute tabular errors. The quantities to apply both to the azimuthal and vertical semi-diameters now in use would be small ; for each observer they would be,—

D =	− 0ˢ·02	or	− 0′·30
E =	− 0·05		− 0·75
C =	− 0·03		− 0·45
J. C =	− 0·10		− 1·50

Before opposition, as the observations of the upper and lower limbs are about equal, the lower limb being observed after the meridian, and the upper limb before the meridian, a decrease in the adopted vertical and horizontal semi-diameters tends to increase *positively* the error of tabular R.A. After opposition, a decrease in the semi-diameter in azimuth tends to diminish the tabular error of R.A., and the lower limb being nearly always observed east of the meridian, the tabular error is also diminished if the semi-diameter be decreased. I have computed, for mean hour-angle and N.P.D. the exact correction due to a decrease of 1″·50 in both the azimuthal and vertical semi-diameters.

The compound effect on tabular error in R.A before opposition is	+0ˢ·14
That due to correction of azimuthal semi-diameter	+0·09
„ „ vertical „	+0·05

After opposition the same numbers will apply with changed signs. If we apply these corrections to the means of Mr. Carpenter's observations in Table I., and proportional corrections to the means of the other observers, the individual mean tabular errors before and after opposition become accordant, although there still remains a visible personality in the absolute errors determined by the different observers.

Before Opposition.	After Opposition.
D = +0ˢ·07	+0ˢ·07
E = −0·04	−0·04
C = +0·04	+0·07
J.C = −0·07	−0·07

Transit Circle.

I have treated the tabular errors in R.A. deduced from the Transit-circle observations in a similar manner to the Altazimuth

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observations. The materials are, however, much fewer, and the intermixture of the observers is not quite so complete. This comparative inferiority, with respect to numbers, is however, in a great measure compensated by the more direct manner with which the tabular errors have been obtained, and by the superior optical power of the instrument. The yearly means of the tabular errors are contained in the next table.

Table IV.

Before Opposition.

Year.	Mean Error of Tabular R.A. by D.	E.	C.	J.C.
	s	s	s	s
1863	−0·204	−0·249	−0·168	−0·249
1864	−0·071	−0·129	−0·025	−0·187
1865	+0·055	−0·072	+0·056	−0·089
1866	+0·131	+0·036	+0·147	+0·027
1867	+0·186	+0·108	+0·245	+0·101
1868	+0·220	+0·158	+0·270	+0·130
Mean	+0·053	−0·025	+0·087	−0·044

After Opposition.

Year.	Mean Error of Tabular R.A. by D.	E.	C.	J.C.
	s	s	s	s
1863	−0·111	−0·060	−0·04	−0·118
1864	+0·096	+0·060	+0·131	+0·060
1865	+0·174	+0·102	+0·168	+0·217
1866	+0·254	+0·136	+0·277	+0·194
1867	+0·350	+0·332	+0·362	+0·375
1868	+0·400	+0·323	+0·423	+0·399
Mean	+0·194	+0·149	+0·226	+0·188

The differences of each observer's mean results from the mean of all are

	Before Opposition.	After Opposition.
	s	s
D =	+0·035	+0·004
E =	−0·043	−0·041
C =	+0·069	+0·036
J. C =	−0·062	+0·002

By taking the difference between the corresponding tabular errors determined before and after opposition in each year, Table IV., it will be seen that in every instance the errors are in excess after full Moon. The mean quantity in excess for the six years is as follows:—

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$$D = +0^s.141$$
$$E = +0^s.174$$
$$C = +0^s.139$$
$$J.C. = +0^s.232$$

The mean of all is $+ 0^s.171$, showing that the tabular duration of the Moon's passage over the meridian deduced from Hansen's Tables should be diminished by about $0^s.085$. This quantity agrees in the same direction with that deduced directly from transits of the first and second limbs at opposition, as given in the Introduction to the *Greenwich Observations* from 1863 to 1868. The next table corresponds with Table III.

Table V.

Before Opposition.

Year.	D.—E.	D.—C.	D.—J.C.
	s	s	s
1863	+0.045	−0.036	+0.045
1864	+0.058	−0.046	+0.116
1865	+0.127	−0.001	+0.144
1866	+0.095	−0.016	+0.104
1867	+0.078	−0.059	+0.085
1868	+0.062	−0.050	+0.090
Mean	+0.078	−0.035	+0.097

After Opposition.

Year.	D.—E.	D.—C.	D.—J.C.
	s	s	s
1863	−0.051	−0.107	+0.007
1864	+0.036	−0.035	+0.036
1865	+0.072	+0.006	−0.043
1866	+0.118	−0.023	+0.060
1867	+0.018	−0.012	−0.025
1868	+0.077	−0.023	+0.001
Mean	+0.045	−0.032	+0.006

With respect to the personal differences between the observers these numbers are confirmatory of those in the corresponding table in the Altazimuth results, but they are of a much smaller magnitude, as might be expected. If we place the means of the two sets side by side, their relative agreement for personality will be better seen.

	Before Opposition.		After Opposition.	
	Altazimuth.	Transit-Circle.	Altazimuth.	Transit-Circle.
	s	s	s	s
D−E	+0.151	+0.078	+0.076	+0.045
D−C	+0.047	−0.035	+0.028	−0.032
D−J.C.	+0.254	+0.097	+0.032	+0.006

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The effect of the personality, whatever may be its cause, is visible in the tabular errors more in the first half of the lunation than in the second, as with the Altazimuth observations, but the differences of each observer's means from the mean of all are too small to trace the personality to any particular source. The result of this investigation has convinced me more than ever of the advantage derived from an intermixture of the observers when absolute places are required. It is now a well-known general fact that a long series of observations made by one observer may appear in most beautiful accord, and yet the mean of all may be considerably in error from the effect of some peculiar personality. On the other hand, where several observers are well intermixed, as in the observations now under discussion, personalities may exist, but their effect is most probably nearly eliminated when the results are grouped together. In personal habits of observation, it is not always easy to determine which observer approaches nearest to absolute truth, and it is very possible, and probable also, that that perfection is never attained even by the oldest or most experienced. Where several trained observers are employed in the same series of observations, it is very likely that the mean of all the observations is the correct result, and that it is nearly free from error arising from personality. But the importance of detecting the small personal differences which are found in almost all astronomical observations, and when discovered, to determine their amount, cannot be overrated. This importance must be my excuse for offering these few remarks to the Society, on a subject which is necessarily somewhat of a technical character, and which is probably interesting in a great measure to the professional astronomer alone.

Greenwich, 1869, *April* 7.

Description of an Improved Driving-Clock.

By Sidney B. Kincaid, Esq.

The failure of the ordinary governor-clock to give in several instances which came under my own notice, when applied to Equatoreals of large size, a motion sufficiently regular for the purposes of ordinary astronomical observations, and its lack of the accuracy necessary for the successful conjunctive use of the spectroscope led me some considerable time since to give my attention to the devising of a mechanical system which should form an apparatus more nearly perfect in its performance. Many and great interruptions have hindered the task and compelled me to delay the completion of my plan. I lay it before the members of the Royal Astronomical Society now, before having it put to the test of actual experiment, because the principle in which it differs from other driving-clocks and the mode of using it can be seen, and their advantages in practice judged of and determined by

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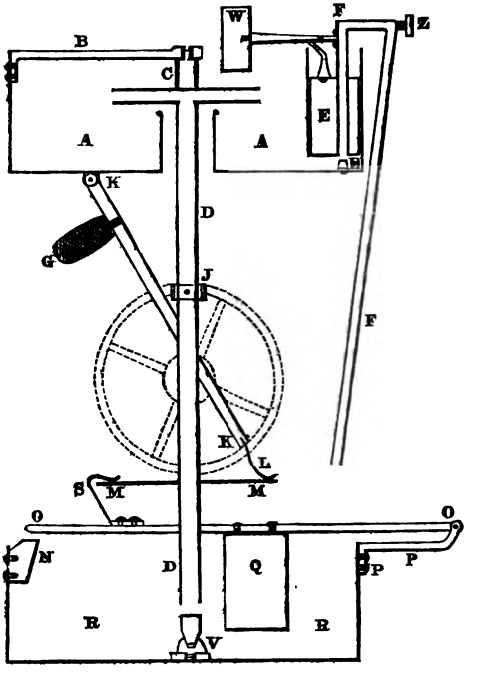
mere inspection of the scheme, and because there may be among the members of the Society some having the opportunity and the disposition to do that which I could not myself undertake for some years.



R is the lower vessel having in the centre at the bottom a conical bearing V for the lower end of the tubular shaft D to rotate in. The upper vessel A is supported over the lower one by columns, not shown in the figure, and carries at the end of an arm B fixed to its side a cylindrical bearing for the upper pivot of D. This shaft D is furnished with apertures into its interior below the surface of the fluid in the lower vessel, and with a pair of of parallel horizontal plates attached near its upper end, so that the space between these plates communicates with the bore of the shaft; the train of wheels, by which the driving-power of the weight is transferred in the usual way to the tangent-screw of the telescope, is connected with the shaft through the vertical crown-wheel, shown by the dotted outline in the diagram, which works with the pinion J on the shaft D. To the shaft is also fixed the plane horizontal concentric disk M, on which rests the steel spring S attached to the lever O which carries the float Q, and plays on a hinge at the end of the arm P fixed to the side of the lower vessel R. To the under side of A is attached by a hinge the rod

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K, so that by moving the handle G upwards the spring L at the end of the rod can be made to press upon the disk M with sufficient force to stop the clock. Between guides in the upper vessel works a float E which carries the siphon F, counterposed by the weight W. H is the conical value to fit the opening of the siphon, so that when, after the clock has been stopped, the float E settles down as the fluid flows out of A through F it closes the opening and leaves the siphon filled with fluid ready for the next start. A convenient substitute for this value and the brake G K L might be found in a stopcock fitted to the longer limb of the siphon. Z is a screw to adjust the flow through the siphon. N is a stop for the end of the lever O.

Suppose now the weight wound up, the siphon filled with fluid, and the disk M relieved from the pressure of the spring L. By pouring fluid into the lower vessel R the float Q will be made to rise with the lever O, thus freeing M from the pressure of the spring S. The descent of the weight will now make the shaft D rotate rapidly, and the centrifugal force developed will exhaust the fluid from the lower vessel R and pour it into the upper one A, from which it will flow back through the siphon F into the lower vessel. Now adjust the screw Z so that when the clock goes at the proper speed the flow through F may be exactly equal to the flow through D. Then, since the velocity of the flow of a fluid under a constant pressure through an orifice of constant size is very nearly constant, and the float E keeping the end of the siphon always submerged to the same depth makes the pressure of the column of fluid in F constant, it follows that the rate of flow through F will always be what the rate of flow through D ought to be. If the speed of the clock is increased more fluid would flow through D than through F, and thus its level in R would fall, and the float Q sinking with the rod O would cause the spring S to press more hardly on M and to thus diminish the speed; if, on the other hand, an increase of resistance in any part retarded the clock, more fluid would flow through F than through D, and the float Q would rise and diminish the pressure of the spring S on M and increase the speed.

It is to be noticed that in this apparatus the regulator is perfectly independent of the driving-train, that every variation from the normal rate is instantly and *absolutely compensated*, and that in constructing such a clock the sensitiveness can be increased by increasing the normal flow.

The greater mobility of a fluid than of any system of mechanical joints made me determine, in the first instance, that it was by the use of a fluid that I should endeavour to attain the object I had in view. Whatever prejudice there may be against the use of a fluid for the purpose is unreasonable, forasmuch as the only objection which could be rationally urged—its liability to freeze, and thus to render the apparatus useless for the time being during the colder seasons of the year—does not apply either to the fluid I myself intended to be used—castor-oil, selected from specimens

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which are not congealable by the winter cold of this country, and such are readily to be met with in commerce—or to that which Mr. Huggins suggested to me, glycerine.

Trinity College, Cambridge,
27 March, 1869.

On the Practical Speed of Electricity through 7200 Miles of Land Wire. By G. Davidson, Esq., Astronomer, United States Coast Survey.

(*Communicated by C. Piazzi Smyth.*)

Observations to the above end were made on the night of Feb. 28, 1869, at San Francisco, California, on a loop of wire extending thence to Cambridge, Massachussets, 3600 miles, and returning to San Francisco; the return signal being received on a chronograph close to that receiving the outgoing signal.

The total interval of time between the records of the two signals amounted to 0·8 of a second; and this may be called the *practical* speed through 7200 miles of land wire, because that length was cut up in the usual service manner of telegraphic companies, into 11 portions, say of 650 miles each; these portions being connected by as many relays (or repeaters, as they are called in America), which send forward on each of these occasions a new electrical current into the next section of the wire; and such new electrical current would travel through its new 650 miles of wire much more quickly than an original current through the second, third, or any subsequent continued length of 650 miles.

The integral result may, therefore, be looked on as a safe and satisfactory multiplication by 11 of the time occupied by electricity in both travelling through 650 miles of land wire, and working *one* relay of the kind, whatever that may be, now used in the United States; the time occupied in effecting such work being apparently 0·07 of a second.

Instrument for Sale.

A very fine Equatoreal, by Cooke of York; almost new. Aperture 6 inches, clock movement, eye-pieces, finder, &c. Also, a Sidereal Clock and Chronometer, by Dent. Address Miss Naylor, Altrincham.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIX. *May* 14, 1869. No. 7.

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Admiral MANNERS, President, in the Chair.

Dr. F. Brünnow, Astronomer Royal for Ireland, Dublin;
Rev. Robert Crowe, Huddersfield;
Thos. Cooke, Esq., Southampton Street and York;
John Knox Laughton, Esq., Royal Naval College, Portsmouth; and
William Locke Lancaster, Esq., Down's Park Road, Hackney,

were balloted for and duly elected Fellows of the Society.

Note on an Aurora Borealis, April 2nd, 1869.
By John J. Plummer, Assistant at the Durham Observatory.

A short time since the idea occurred to me that something might be added to our knowledge of the nature of the Aurora Borealis, by means of the analysis of its light by the spectroscope, and I resolved to examine its spectrum upon the first opportunity. A very fine Aurora was visible at Durham, soon after midnight on April 2nd. It is the result of the observations made upon that occasion, that I have now the honour to communicate to the Society.

The light of the Aurora appeared to the eye as white, perhaps slightly bluish, and not of the more common ruddy hue; there were few or no rays, but broad sheets or waves of light succeeded each other rapidly, proceeding from the arch to a height of about 30° from the horizon. When the light was examined by the spectroscope, one *bright* line was immediately seen. At the brightest parts of the arch, this line was perhaps equal in intensity to that of the most conspicuous of the lines in the Nebula of *Orion*, it presented ordinarily a somewhat hazy appearance, but at times would flash out sharply and well defined. It was difficult to make any accurate measurement of its position, but two approximate mea-

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sures were taken, by illuminating the field suddenly, by a hand-lamp and placing the cross-wires as nearly as possible on the position in which the line had been seen. These two estimations are respectively $19^{rev.}\cdot40$ and $19^{rev.}\cdot25$.

The following are the readings of the principal Solar lines, as measured on March 6th, 1869, for referring these numbers to their places in the spectrum:—

rev.	rev.	rev.	rev.	rev.
A = 14·909	*a* = 15·397	B = 15·934	C = 16·496	D = 18·000
B = 19·961	*b* = 20·357	F = 21·751	G = 25·307	H = 28·503

The two conspicuous pairs of lines in the air spectrum near D, would be represented on this scale by the numbers $17^{rev.}\cdot89$ and $18^{rev.}\cdot61$; but I cannot believe that the two aforesaid estimations are so much in error that the latter of these lines is the one seen in the spectrum of the Aurora. I am, therefore, led to the conclusion that the spectrum of the Aurora is *not* identical with that of air; but as some doubt may exist on this point, I shall anxiously await the next apparition of an Aurora to verify this result. The advanced season, however, renders it unlikely that an opportunity will occur for some months. I have, therefore, thought it right to communicate my present results, however imperfect they may be.

It may be worthy of remark that the line in the Aurora spectrum agrees closely with that of the more conspicuous of the lines in the spectrum of *α Orionis* between the solar lines D and E. I have measured this line upon two occasions, Feb. 12 and Feb. 13, and the resulting measures were $19^{rev.}\cdot350$ and $19^{rev.}\cdot381$ respectively. There is also a tolerably conspicuous line in *Aldebaran*, near the same place. It may be also necessary to state that all these measurements have been referred directly to the sodium line D, by the observation of the spectrum of the metal from the salt with an alcohol flame, both before and after the observations. They are thus strictly comparable.

Durham Observatory,
April 6th, 1869.

Addendum.

Since the above paper was written, I have been informed by Mr. Huggins, that a similar observation has been made by Angström, and confirmed by Struve, during the winter 1867–68. I was totally unaware of this at the time of communicating the above paper. I will add that I have also had an opportunity of confirming my own observation. An aurora of surpassing magnificence was visible in the North of England upon May 13; a corona was formed, and every part of the sky for a short time was filled with streamers. Some of these, in the West and South-east especially, were of a deep red colour, and the single line in

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the spectrum was seen here precisely as in the white parts of the aurora. I succeeded in making one good estimation of the position of the line from the red streamers, sufficient to identify it with that of the other measures made later on the white arch which was formed in the North-west after the corona and streamers had partially or wholly disappeared. This estimation gave $19^{rev.}\cdot18$ as the position of the line. The subsequent measures give mean results of $19^{rev.}\cdot03$ and $19^{rev.}\cdot10$ respectively, which are believed to be tolerably close approximations, though not quite so satisfactory as had been hoped for.

No other lines were seen.

The extreme tenuity of the matter forming the aurora was demonstrated by the fact that Winnecke's Comet was visible with the $6\frac{1}{2}$-inch refractor of the Observatory, through one of the densest streamers, without any other inconvenience than that of the brightness of the field of view.

Durham Observatory, May 17th, 1869.

On the Determination of Longitudes by Electric Telegraph. By E. D. Ashe, Commander R.N., Director of the Observatory, Quebec.

As I have had some experience in determining the longitude by electric telegraph, and, as it is likely to be useful to several of our Members in different parts of the world, I send an account of fixing the position "Rivière du Loup."

Sir William Logan (Director of the Geological Survey) requested me to obtain the latitude and longitude of "Rivière du Loup," which is about 130 miles below Quebec.

I started by railway on the 4th February; took with me a 24-inch transit; and a sidereal chronometer.

On my arrival, I called on the telegraph operator, who I found could not read by sound, and, consequently, I resolved to fix my transit-instrument in his garden, so that I should have only to take the chronometer into his office when I was ready to send signals.

I generally select a boulder, or get a large stone, upon which I place the instrument; erect a hut to keep the wind off; lead the telegraph wire into it; and, with a little portable telegraph-instrument that I always carry with me, I have all that is required when I am ready to send signals.

In this instance all the Earth was covered with snow for several feet in depth, and nothing like a stone for the support could be obtained; so I got a flour-barrel, removed the snow from the earth, and placed the barrel on the ground; then filled it with sand, poured two or three buckets of water over it and around it, and, as it was freezing, placed a square piece of board on the top; and as the thermometer was several degrees below

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zero, in a few minutes the whole was one solid mass. I then with boards built a hut.

I had to wait several days for a clear night, when I got the instrument into the meridian. As the country was covered with snow, and as all the fences are rails, you are sure of getting a "meridian mark" in some of the many posts. I had one about four miles off, a black line upon a white ground.

I have frequently been annoyed by boys, particularly in Upper Canada, where they are of the English type, who, when they see a light, commence throwing stones, and it is impossible to count seconds under those circumstances. On this occasion I heard one night a boy's voice, saying, "Please, sir, will you let any body in?" "No! but you may look through the crack." I cannot say if he took advantage of my kind offer; but if he did, he was as quiet as a mouse.

I got both latitude and longitude in the first four clear nights. After I had sent "time" to Quebec, I removed the transit-instrument, and drew a circle through the three points made by the three supports of the instrument, and moved one of the points 90° on the circle, and then I had the instrument on the prime vertical for latitude.

I was detained 19 days at the hotel waiting for clear weather. The expenses for travelling, building hut, paying operators at both ends of the line, amounted to 13*l.* This does not include remuneration for myself.

Quebec sent "taps" from the "mean time clock," commencing at the even minute, and continuing beating seconds until the 50 second in each minute, and this they did for six minutes. "Rivière du Loup" noted the "coincident tap" with the sidereal chronometer, marking the second and the series; and thus we had the difference of longitude to the hundredth of a second at once.

"Rivière du Loup" then sent "sidereal seconds" to Quebec, and this was repeated twice from each end of the line.

I cannot help thinking that this plan is the easiest and least troublesome, only requiring the telegraph line for half an hour, which is of great consequence.

The Latitude by Transit in Prime Vertical.

	°	′	″
α Aurigæ ..	47	49	44
1071 B.A.C. ..	47	49	45
			89
Latitude of Rivière du Loup	47	49	44·5

At Quebec 11 stars were observed for clock error,— 7 stars, lamp west, and 4 stars, lamp east; deviation obtained by *Polaris* and ε *Ursæ Minoris*, and level frequently applied.

At Rivière du Loup 13 stars were observed,— 7 lamp east and 6 lamp west. Deviation by *Polaris* and ε *Leporis;* and level frequently applied.

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1st Set of Signals from Quebec.

	h	m	s
Coincident tap sent by Q. at ..	8	52	10·00
Clock fast			−45·37
Mean Time at Q.	8	51	24·63
Ditto at R. du Loup	8	58	3·37
Diff. Long.	0	6	38·74
Coincident tap noted at R. du Loup	5	18	35·00
Clock slow		+1	1·71
Sid. Time	5	19	36·71
Ditto at Mean Noon	20	20	5·04
Ditto since Mean Noon	8	59	31·67
Retardation		1	28·30
	8	58	3·37

1st Set from Rivière du Loup.

	h	m	s
Coincident tap noted by Q. at	9	28	5·00
Clock fast			−45·37
Mean Time at Q.	9	27	19·63
Ditto at R. du Loup	9	33	58·38
Diff. Long. 2d Set		6	38·75
,, 1st Set		6	38·74
Coincident tap sent by R. du Loup	5	54	36·00
Clock slow		+1	1·71
Sid. Time at R. du Loup ..	5	55	37·71
Ditto at Mean Noon	20	20	5·04
	9	35	32·67
Retardation		1	34·29
	9	33	58·38

The next two sets of signals were turned into sidereal time, giving —

	h	m	s
A Mean Difference of Longitude of	0	6	38·70
And the Grand Mean of the 4 Sets was	0	6	38·725
Longitude of Quebec	4	44	49·02
Longitude of Rivière du Loup ..	4	38	10·295

Observatory, Quebec, March 4, 1869.

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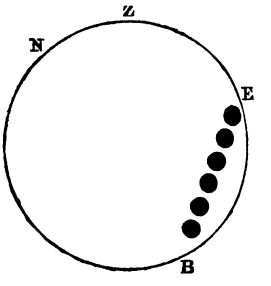
On the Transit of Mercury over the Sun's Disk on 5th Nov. 1868, *as seen at Vizagapatam.*

(*In a Letter from A. V. Nursing Row, of Vizagapatam, to Prof. C. Piazzi Smyth, Edinburgh.*)

The annexed diagram represents the disk of the Sun at the time of the Transit, as visible to an observer at Vizagapatam, for direct image.



Z represents the vertex, or the highest point of the Sun's disk.

N the north point.

B the point of first contact.

E the point of last contact.

	Mean Time. h m s
First contact	10 58 39
Last contact	2 35 57

The dark spots in the diagram represent the dark disk of *Mercury* when crossing over the Sun's disk.

"I have the pleasure to enclose herein a copy of a printed notice to friends in this neighbourhood, containing a rough diagram of the Transit of *Mercury* as it was seen here in Vizagapatam. The weather certainly favoured us on the 5th of November, and we enjoyed well the observation of the Transit. The planet was seen as an intensely dark spot on the Sun's disk. When the planet approached half-way of the transit, some of my European friends and myself observed a wavy tint of light darting from the upper edge, disturbed at times, but continued until the planet had passed some distance from the highest point of the line of transit.

"Suspecting it to be an effect of disturbed focus, I often refocussed the instrument and changed the eye-pieces, besides other precautions, yet we always observed the same phenomenon, but with high powers we found the edge of the planet was not so intense as in the centre, and especially so at the line of light in question. The telescope we used was the one which I explained in my pamphlet on the recent solar eclipse, and shows the stars without wings, &c., therefore I am very anxious to be enlightened on the cause of such appearance, if it were not in any way an optical delusion.

"A scientific friend who was then present remarked to me that he was informed somewhere that the planet, when leaving the Sun's disk, would have the appearance of a flask. I tried my best and watched for it, but did not find with my instrument anything but a dent on the edge of the Sun's disk at the last contact."

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Note by C. P. S.—The writer of the above extract is a Hindoo gentleman of Vizagapatam, whose family has been much given to science through two generations. He possesses an extensive Observatory, both astronomical and meteorological, and is now about adding to the former a 6-inch Equatoreal, with driving-clock, micrometer, and spectroscope; and to his library all the purchasable volumes of the *Monthly Notices of the Royal Astronomical Society*.

The telescope with which the above observations were made has an object-glass of 4·8-inches aperture, by the late W. S. Jones, of London, and was fitted up in Mr. Nursing Row's own workshop on a large and original kind of altazimuth stand, supplied with vernier-circles, levels, and slow-motion screw-movements. In his solar-eclipse pamphlet of last August, Mr. Row thus alludes to the telescope:—

"As to the character of the object-glass of the present telescope I may add that it gives no colour around the image of any object; and that the image of any fixed star it shows is free from the optical appendages of wig or wings. It can be judged from this that the present telescope will discharge its duties faithfully."

The failure of Mr. Row to see the irradiation-distorted figure of the planet *Mercury* is probably due to his appearing, from his description, to have expected to see it, not at the internal, but at the external contact of Sun and planet. What, however, the *wavy* phenomenon was which he saw so persistently during the middle of the transit I do not pretend to suggest any explanation of.

On a Method of Imitating the Transit of an Inferior Planet.

By H. W. Hollis, Esq.

At the close of the November Meeting of the Royal Astronomical Society, I mentioned to the President an experiment which I had made with a view to obtaining artificially the appearances presented immediately after the first or before the last internal contact at the transit of an inferior planet. I was led to make this experiment from having observed, at the transit of *Mercury* on the 5th November last, the formation of the black connecting band or ligament between the planet and the Sun's limb, immediately before the last internal contact. On that occasion I found it quite impossible, on account of the formation of such apparent connecting band, to say within several seconds when the real internal contact took place.

At the top of a tower 150 feet high, I placed a projecting spar of wood, and from the outer end of this spar I suspended by a fine wire a lead ball, half an inch in diameter. I then went to a position upon the ground, 286 yards distant from the ball, which, from that position, would subtend an angle of about 10 seconds, or nearly the apparent diameter of *Mercury* at transit.

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I waited until, by the effect of the Earth's rotation, the ball was seen projected upon the Sun's disk, and upon then observing the ball with a 6-inch refractor, and power 220, I obtained, at its passing off the disk, an exact representation of the formation of the black connecting ligament. Of course, the duration of this artificial transit was very short, and the apparent motion of the ball across the disk, or rather, of the disk behind the ball, was such as to render the approach of the ball to the limb infinitely more rapid than the corresponding appearance in the real phenomenon, and the ligament was only visible for a second or two, but still it was sufficiently distinct to render the experiment an interesting one.

On mentioning this experiment to the President, I was requested by him to give a written description of it, to be laid before a subsequent Meeting; and I now do this the more willingly from having observed that the possibility of making such an imitation of a transit was referred to at the last Meeting.

Keele, Newcastle, Staffordshire,
5th May, 1869.

Note on the Preparations desirable for Photographic Observations of Phenomena such as Transits of Venus. By Major Tennant.

Mr. De La Rue has in the *Monthly Notices* for December last again brought forward the claims of Photography to be used in permanently recording for examination at leisure the phenomena of the Transits of *Venus* in 1874 and 1882. I believe the probable value of this method is very great, though I feel that evidence has still to be adduced on this subject before it can be unreservedly trusted; and I propose here offering some suggestions as to apparatus and procedure, in the hope of inducing a fair trial.

First. It appears to me that an effort should be made to get rid of instrumental distortion; for I think the necessity for investigation of its amount and of applying corrections a serious drawback. If a reflecting telescope with *unsilvered* glass mirrors were used, the light would be very much diminished; and still more so if the image were optically enlarged. In a Newtonian telescope this might be done by either a convex or concave lens achromatised for the actinic rays, of which arrangement the latter would make the apparatus more compact; or the telescope might be a Cassegrain, thus becoming still more compact and manageable. An instantaneous shutter would in any of these cases allow a fairly large aperture to be used, and this having its centre part removed would give good definition.

Next. I would make a modification in the way of releasing the shutter. At Kew it is held against a spring by a thread which is cut with scissors. I would propose that it be retained by an electromagnet, and that the current forming this should also pass through

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a chronograph. If an observer at a separate telescope had a break circuit key he could at any moment photographically record a phenomenon he saw and the instant of its occurrence. An assistant at the heliograph would be needed to replace the shutter and insert the dark slides, unless the observations are far apart. By using a repeating slide and combining the measurements on one plate, very valuable normal relations of the Sun and planet would be found, and observations might be made during the ingress or egress (possibly two of the black drop) in considerable numbers.

There is an advantage in this mode of observing the transit of *Venus* to which Mr. De La Rue has not alluded.* If accurate micrometrical observations can be made by means of photographic pictures, then the range of suitable stations can be greatly enlarged; for any two stations, 140° or 150° apart, can have the observations combined by choosing a suitable time, and, of course, by means of equations of condition, the observations of stations in all sorts of places could be used with their proper weight. All the photographs could be measured by the same micrometer, whose errors of all sorts could be very carefully examined.

Before, however, it could be expected that reliance should be placed on these photographic results, it is clearly necessary that a rigorous examination should be instituted of the forms of apparatus &c. An image formed, as that in Mr. De La Rue's apparatus at Cranford, or mine at Guntoor, is evidently almost free from distortion, save those arising from the collodion film. It is small, and would admit of measurement to great accuracy with a micrometer-screw of fine pitch. If enlarged by any of the means before described, coarser means of measurement would produce results equally accurate, but the distortion of the enlarging lens would be involved. Lastly, the small images might be enlarged by a specially made lens (as in the case of the Guntoor photographs), and these might be measured. In some respects this method would be better than the last, as the original images might be better centred in the axis of the enlarging lens, which, too, would be of longer focus.

All these methods must, to produce confidence, be tried. Probably as convenient a method as any would be to take photographs in various ways of any nearly total eclipse, and to treat them rigorously as micrometrical measures would be treated in an observatory, deducing the probable errors of one observation in each way.

I believe (for my own part) that the results would prove photograph registration to be thoroughly reliable as a substitute for

* Mr. De La Rue has pointed out the facility with which the nearest approach can be got from the photographs at any one station, but if photographs be taken at two stations at a time when *Venus* is in the plane which includes them both as well as the Earth's centre, these will show the whole effect of parallax; and it is the positions at these instants, and not the nearest approaches to the Sun's centre, which should be compared.

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micrometrical measurements in the ordinary way, and possibly they might be much better. Even if the suitability were disproved, it would be of some value and might save a waste of energy during the transit of *Venus*.

If it be determined to send parties to photograph during the next total eclipse in the south of India, then a Photoheliograph might be placed at the centre as well as at either edge of the shadow-path as well as at the apparatus for the total phase only, and a very complete examination of the question at issue would be possible.

Mooltan, March 22, 1869.

Comments on Major Tennant's Note on the Preparations for Photographing the Transits of Venus. By Warren De La Rue, Esq.

I beg leave to offer a few remarks on some points in Major Tennant's communication.

1st. With respect to the choice of instruments; a reflecting telescope with unsilvered mirrors would certainly present the advantage over chemically achromatised refractors, in which, as in the Kew Photoheliograph, the image is enlarged by a secondary lens, of giving an image free from optical distortion; that is to say, the Cassegrainian and Gregorian present these advantages, but not so the Newtonian, if an achromatised lens is to be used to enlarge the image before it falls on the sensitive plate. My experience has proved to me that the necessity for obtaining a flat field so complicates the problem that hitherto no lens has been made which answers the conditions to be solved.

Now with respect to the Cassegrainian and Gregorian forms of instrument; it is of course necessary to make a provision for the free transmission and dispersion of that portion of the light which passes through the mirrors; they would have necessarily to be supported in cells open at the back, and their back surfaces would be advantageously made concave (convex towards the first or reflecting surface), in order to prevent any light being reflected by the back of the mirrors and forming a secondary image. The heat emerging from the back of the principal mirror would seriously interfere with successful photographic manipulations, and, although it is conceivable that special provisions could be made to diminish this inconvenience by reflecting aside the transmitted rays, it would complicate the apparatus.

By employing a Newtonian without an enlarging lens the image would be considerably smaller, but it would be free from optical distortion, and by the use of a Herschelian prism the light would in this case be reduced by a second reflection from glass (as in the other forms of reflectors), but the operator would be clear of any of the refracted heat rays, and would work at ease. I give, therefore, the preference to the Newtonian.

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I do not think, however, that the reflector is on the whole so convenient an instrument for observing transits of *Venus* as a refractor on the Kew model; for the optical distortion of such refracting instruments can be ascertained practically and allowed for, without in any way interfering with the final correctness of the deduced results.* Moreover, strictly speaking, the reflector is not absolutely free from optical distortion on account of the photographic plate having a flat and not a curved surface.

2nd. As regards the measuring instrument I am afraid that I have failed to convey a correct idea of the capabilities of the micrometer I devised for measuring the eclipse-pictures of 1860, and which I have described and figured in the *Phil. Trans.* of 1862, pages 373–380. Major Tennant evidently thinks that the instrument is only calculated to deal with quantities no smaller than the $\frac{1}{1000}$th to the $\frac{1}{2000}$th of an inch, representing when used in conjunction with the Kew Photoheliograph 0″·5 to 0″·25; and that the substitution of a micrometer screw for the scale of equal parts and Vernier would be an improvement. The instrument is really provided with a micrometer screw, which may be used for making the measurements of any required minuteness independently of the scale of equal parts, which was added for the special work in hand, namely, for the measurement of pictures already enlarged by the secondary magnifier to four inches in diameter. The clamping apparatus for throwing the micrometer screw in and out of action is also a special provision for the measurements of the actual Sun pictures, for it would be exceeding fatiguing to give several hundred rotations to the screw whenever a measurement had to be made. There really is no limit to dimensions of the instrument, which is adapted for the measurement of any rectangular or other co-ordinates or any angular quantities which may be desired, and by increasing the optical power of the microscope it may be used for measuring astronomical photographs with any degree of precision for the special requirements of the case. It would be quite unnecessary, and indeed detrimental, to enlarge photographs for micrometrical measurement when once they have been procured by any form of heliograph.

3rdly. With respect to the localities to be selected, the employment of the photographic method of observing transits of *Venus* has, as I have stated in the *Monthly Notices*, vol. xxix. No. 2, page 49, the advantage of rendering us independent of conditions favourable or indeed essential for eye-observations; for a few photographs obtained at each station would afford the means of ascertaining the path of the planet and its position at any given moment; and hence the proposal to determine the position of the planet's centre in relation to the Sun's centre really includes the particular case contemplated by Major Tennant in the foot-note appended to his paper; but it is possible that the Sun might be obscured at the critical time, at one of the two stations selected.

* *Monthly Notices*, vol. xxix., p. 51.

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I did not describe the method of ascertaining the position-angle of the planet (at p. 51 of *The Monthly Notices*, vol. xxix.) because the means of determining the position-angle of wires in the foci of the primary and secondary lenses of the Kew Photoheliograph has been fully described in the *Phil. Trans.* for 1862. I may mention, however, that it can be ascertained within 1′ of arc.

On the Solar Eclipse of 1871. By Major Tennant.

(*Extract from a Letter to Mr. De La Rue.*)

I sent last mail to Mr. Airy a proposition to superintend three parties in the south of India in the Eclipse of 1871. I fear he will think it far too extravagant for recommendation. The idea of three parties is I believe yours; I was sorry there were not three last August, but in that stage of the matter it was hopeless, and the point I lay most stress on is that each party who go to photograph the obscuration should have a Photoheliograph to make use of, and to try rigorously by purely astronomical tests. A small probable error for the measures from a picture would be conclusive, unless it was shown that all would have a common error. Now if two pictures could be taken on one glass, these *could not have* any common error. If the pictures were taken in the principal focus of the telescope there would be no distortion except a very minute amount arising from equal parts of the image not corresponding to equal angles. This could be corrected (if needed at all) by mere theory.

I have not added in the proposition what is I think important, and that is, an examination of the Corona Spectrum with a large telescope, and arrangements to show whether it has any lines. The cost of this would be a good deal. The telescope *would not need equatoreal mounting* if an assistant were available, but a spare assistant would probably be equally costly, and he would need, as in M. Janssen's apparatus, a good large telescope himself. I should, too, like to be further on with other things before looking at this. I should not undertake it (if I had three parties) till I were sure of the leaders at all events.

I wish I could give some estimate, but that I cannot. Using chronometers and leaving out clock &c., would save, but I should prefer the other plan, and I do not look to it in any sense as all money spent on the Eclipse, for a large part would clearly be money spent in explanatory experiment as to the applicability of Photography in 1871 to delicate astronomical purposes.

If three Photoheliographs at stations near each other, whose absolute and relative place are well known, give accordant values for the tabular errors of all sorts, and assign a small probable error to one observation or photograph, then I think the matter will be settled beyond dispute, and I believe, if proper care be taken, the result will be decisive.

I cannot be at home to start it, and must hope that some of

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my friends will urge the matter. You have yourself taken so much interest in the application of Photography that I know you will aid if you approve of the means. If it is to be carried out by *me* I should be got to England as soon as possible, and I certainly say I shall have no objection to an early start. Three months, however, would probably elapse between the Secretary of State making up his mind and my reaching England, and that is a very sensible portion of what is left for preparations.

On the Solar Eclipse of August, 1869.

By R. T. Paine, Esq.

(*Extract from a letter to the Secretaries of the Society.*)

You are aware that on the 7th of next August the shadow of the Moon will pass over North America, and that, on that day, in some of our *Atlantic* States there will be a total eclipse of the Sun. It will be the fourth within ninety-one years, and the third in this century, and there will be only one more before 1901 (May 28th, 1900), unless we include Texas in the *Atlantic* States, over which the shadow will pass in the fourth return of our great eclipse of June 16, 1806, on July 29th, 1878.

As such great interest has of late years been taken in these phenomena, and as the eclipse of next August is the first return of the one so successfully observed by many English astronomers, in July, 1851, in Sweden, it has seemed to me as very probable that some of them would visit North America for the purpose of observing also the one now approaching; hoping that this opinion may prove correct, I take the liberty herewith to transmit a computation of the path of the centre of the Moon's shadow, and to make the request that you would deliver it to any one who may contemplate undertaking this voyage, and to whom it may be acceptable. This computation was made by myself some months ago, for my own information and has not been made public. It was made from the longitude, latitude, and other elements of the Sun and Moon, as very carefully deduced from the English *Nautical Almanac*, except that for Encke's Solar Parallax on August 7th, the quantity $8''\cdot83$ was used.

The duration of totality when longest (near Mount St. Elias) will be quite four minutes, but it gradually diminishes to $3\frac{1}{2}$ ($3^m\ 28^s\cdot4$) at our boundary line, latitude 49°; at the City of Des Moines, the capital of Iowa, it will be exactly three minutes; at Springfield, the capital of Illinois, $2^m\ 51^s$, and about 30 miles S.W. of Raleigh, North Carolina, $2^m\ 27^s$. The territories of Montana and Dakota are we believe, like British America, on the central line, thinly inhabited, but Fort Union on the Missouri (which is reached by steamboats from St. Louis) appears by the map to be very near that line, and, therefore, if correctly laid down, is favourably situated, also in Iowa, the cities of Des Moines, Boonsboro', and Burlington, are all under or very near

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that line; and although distant 1400 or 1500 miles from us, are reached in three days, on continuous railroads from this city and New York, whilst Macomb and Springfield in Illinois, New Albany, in Indiana, and Louisville in Kentucky, equally well situated, and at the distance of about 1000 miles, can be reached in two days, and Abingdon, in Virginia, and Raleigh, in North Carolina, in one day from Washington City, from which they are distant less than 400 miles by railroad.

Now that we are told it is not impossible to see the red flames around the Sun, even when it is not eclipsed, it seems strange they have not always been seen when the obscuration by the Moon was complete. Indeed, the first good account of them I had from an actual observer, was only 18 years ago, from my lamented friend, the late Prof. G. P. Bond, who saw them in Sweden during the totality of July, 1851. I believe I may say they were not noticed by the late Dr. Bowditch, who observed with great care the eclipse of 1806, which occurred here, on a beautiful day in June, with a totality of five minutes, only half an hour from noon, and certainly I did not perceive them in the total obscuration of November 30, 1834, at Beaufort, in South Carolina, where it was central. The duration of totality was to be sure only little more than a third of that here in 1806 ($1^m\ 49^s{\cdot}6$). The second contact I closely watched through my telescope with a power of 40, and a double (red and green) screen, but saw no flames; as soon as possible I read off, and recorded with a lantern the time by a chronometer, and then with an opera-glass looked at the corona and saw that magnificent and appalling spectacle, and at the same time the star *Antares*, that day in conjunction with the Sun, and therefore distant only by its latitude, $4\frac{1}{3}$ degrees. The sky was very clear throughout the eclipse, and the edges of the Sun and Moon sharply defined, and without any apparent distortion whatever.

So much was said last year in the magazines and newspapers about the length of totality ($6^m\ 48^s$) where the eclipse of August 18 was central at noon, which was called "unequalled in centuries past and to come," that merely from curiosity I was led to look into the subject, and the result of my examination thereof was published in a communication in one of our papers, a copy of which I take this opportunity to transmit to you. Since last August I have carefully recomputed, by the tables of Carlini and Burckhardt, but using the Greenwich semi-diameter of the Sun, and Hansen's of the Moon, the second and third contacts of the eclipse of August 7, 1850, for latitude 17° 50′ N. longitude 141° 49′·4, W. where it was central on the meridian, and found they took place as follows:

	G.M.T. h m s	L.M.T. h m s	
Second contact	9 22 55·3	11 55 37·7	A.M
Apparent Conj. of ☉ and ☽ in Longitude	0 26 50·9	0 59 3·3	
Third contact	0 29 46·7	0 2 29·1	P.M.

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	Moon's Ap. Long. — Sun.	Moon's Ap. Lat. — Sun.	Moon's Ap. S.D. — Sun.
Second Contact	−72″·98	+3″·60	+73″·07
Ap. Conj. of ☉ and ☽ in Long. ..	0·00	+0·23	
Third contact	+73·01	−3·14	+73·08

Altitude of Sun and Moon at meridian 88½°; duration of totality, 6m 51s·4, or 3½s longer (if I am correct) than in August 1868. The matter is, however, one merely of curiosity, and may, therefore, be considered as of little importance. I think, however, we may say that in one or two former returns of the series of eclipses to which that of last August belongs, the duration of totality, where central at noon, was quite as long as it was then.

The rapid increase of interest in astronomical phenomena is highly gratifying; perhaps there is no stronger evidence of the rapidity of this increase than that afforded by the degree of attention paid to several of the last, and to the last returns, of the above series of eclipses, which is remarkable, indeed, for the length of totality, where these eclipses are central. The returns of June 20, 1778 (central in United States), and those of July 1796 and 1814, took place, it is believed, with little if any notice; and although at the return of July 27, 1832, only thirty-six years ago, the Moon's shadow passed at about 8 A.M., over two of those quite *near* islands, the Bahamas, viz. Great Inagua and Grand Turk (the latter in N. latitude about 21½°, W. longitude 71½°), and caused a totality of quite five minutes, it did so, it is supposed, without a single scientific observer. In February of the preceding year I saw in the south-eastern part of this State, the first of the four *ring* eclipses I have witnessed, and was thereby rendered desirous of beholding a total obscuration, but the want of a direct communication induced me to forego that pleasure at that time: probably the same difficulty was experienced by those European astronomers who wished to go to Africa or the Bahamas for the same purpose, and the difficulty was then considered insuperable, but it would have easily been overcome by a small part of the zeal and enthusiasm displayed by the observers in India in August, 1868.

Path of the Centre of the Moon's Shadow over the Earth, Saturday, August 17, 1869, *according to Elements of the Sun and Moon as given by the Tables of Le Verrier and Hansen in the English Nautical Almanac, except that for Encke's Parallax of the Sun on August* 17, 8″·83 *was used. Ellipticity of the Earth* $\frac{1}{300}$*th.*

	G.M.T.	L.M.T.	Latitude.	Longitude.
Central Eclipse begins at	8h 46m 8·6s P.M. and	4h 35m 56·6s A.M.	52° 43′·7 N.	242° 33′·0 W.
,, ends	11 15 48·5	6 45 54·5 P.M.	31 17·2	67 28·5
Greatest N. Lat. of C. E.	9 18 30	9 40 14·0 A.M.	64 15·7	174 34·0
Eclipse central at Noon	9 45 59·7	0 5 26·1 noon	61 49·4	145 8·4
Duration of Central Eclipse	2 29 39·9.			

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Eclipse Central at

Siberia.

G.M.T. h m s	L.M.T. h m	Latitude N. ° ′	Longitude. ° ′
8 46 8·6	4 35·94 A.M.	52 43·7 N.	242 33·0 W.
47 0	5 13·19	55 29·3	233 27·1
48 0	32·25	56 45·2	228 56·3
50 0	59·95	58 24·3	222 30·8
52 0	6 22·31	59 34·6	217 25·4
54 0	41·97	60 29·6	213 0·5
56 0	7 0·04	61 14·3	208 59·4
58 0	16·98	51·5	205 15·3
9 0 0	33·09	62 22·7	201 43·6
2 0	48·54	48·8	198 21·9
4 0	8 3·45	63 10·7	195 8·2
6 0	17·90	28·7	192 1·5
8 0	31·94	43·4	189 0·9
10 0	45·62	55·0	186 5·7
12 0	58·97	64 3·7	183 15·5
14 0	9 11·99	10·0	180 30·1
16 0	24·72	13·9	177 49·2
18 0	37·17	15·6	175 12·5
20 0	49·34	13·1	172 39·9
22 0	10 1·25	12·8	170 11·3

North America.

9 24 0	10 12·89	64 8·7	167 46·6
26 0	24·28	2·8	165 25·8
28 0	35·42	63 55·3	163 8·7
30 0	46·31	46·3	160 55·3
32 0	56·97	35·9	158 45·4
34 0	11 7·41	24·2	156 38·9
36 0	17·61	11·1	154 35·9
38 0	27·59	62 56·9	152 36·1
40 0	37·36	41·6	150 39·6
42 0	46·92	25·2	148 46·2
44 0	56·29 A.M.	7·7	146 55·7
46 0	0 5·46 P.M.	61 49·3	145 8·1
48 0	14·45	30·0	143 23·3
50 0	23·25	9·9	141 41·2
52 0	31·89	60 48·9	140 1·7
54 0	40·36	27·1	138 24·6
56 0	48·68	4·5	136 49·8
58 0	56·85	59 41·2	135 17·3
10 0 0	1 4·88	17·2	133 46·8

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G.M.T. h m s	L.M.T. h m	Latitude N. ° ′	Longitude W ° ′
10 2 0	2 12·78	58 52·5	132 18·3
4 0	20·55	27·2	130 51·7
6 0	28·21	1·2	129 26·9
8 0	35·75	57 34·7	128 3·8
10 0	43·18	7·5	126 42·3
12 0	50·52	56 39·7	125 22·2
14 9	57·77	14·4	124 3·4
16 0	2 4·95	55 42·4	122 45·8
18 0	12·06	12·9	121 29·1
20 0	19·11	54 42·9	120 13·4
22 0	26·10	12·3	118 58·5
24 0	33·04	53 41·1	117 44·4
26 0	39·93	9·3	116 31·0
28 0	46·79	52 36·9	115 18·1
30 0	53·63	4·0	114 5·5
32 0	3 0·46	51 30·4	112 53·1
34 0	7·28	50 56·2	111 40·8
36 0	14·12	21·4	110 28·2
38 0	20·98	49 46·0	109 15·3
40 0	27·87	9·7	108 1·9
40 30	29·60	0·4	107 43·5

United States : Territory of Montana.

10 41 0	3 31·33 P.M.	48 51·3	107 25·1
42 0	34·79	32·7	106 48·1
43 0	38·28	13·8	10·8
44 0	41·78	47 54·8	105 33·3
45 0	45·30	35·5	104 55·5
46 0	48·83	16·0	17·5
47 0	52·40	46 56·3	103 39·0
48 0	55·99	36·4	0·1
49 0	3 59·61	16·2	102 20·8
50 0	4 3·27	45 55·7	101 41·0
51 0	6·96	35·0	0·6

Territory of Dakota.

10 52 0	4 10·69	45 13·9	100 19·7
53 0	14·45	44 52·5	99 38·2
54 0	18·27	30·8	98 56·0
55 0	22·13	8·7	13·0
56 0	26·06	43 46·3	97 29·1
57 0	30·05	23·5	96 44·3

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State of Iowa.

G.M.T. h m s	L.M.T. h m	Latitude N. ° ′	Longitude W. ° ′
10 58 0	4 34·11 P.M.	43 0·1	95 58·4
59 0	38·25	42 36·3	11·3
11 0 0	42·47	12·0	94 22·9
0 30	44·63	41 59·7	93 58·1
1 0	46·80	47·2	33·0
30	49·01	34·6	7·4
2 0	51·24	21·8	92 41·4
30	53·51	8·8	14·9
3 0	55·81	40 55·6	91 47·9

Illinois.

11 3 30	4 58·13	40 42·3	91 20·5
4 0	5 0·50	28·7	90 52·5
30	2·91	14·9	23·9
5 0	5·35	1·0	89 54·7
30	7·85	39 46·8	24·7
6 0	10·40	32·3	88 54·0
30	13·01	17·5	22·4
7 0	15·67	2·4	87 49·9

Indiana.

11 7 30	5 18·39	38 47·1	87 16·6
8 0	21·19	31·3	86 42·1
30	24·08	15·1	6·3

Kentucky.

11 9 0	5 27·05	37 58·6	85 29·2
30	30·13	41·6	84 50·6
10 0	33·31	24·0	10·4
30	36·61	5·8	83 28·3

S.W. Corner of Virginia.

11 11 0	5 40·06	36 46·9	82 44·1
15	41·84	37·2	21·1

North Carolina.

11 11 30	5 43·68	36 27·3	81 57·3
45	45·56	17·1	32·8
12 0	47·51	6·6	7·4

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G.M.T. h m s	L.M.T. h m	Latitude N. ° ′	Longitude W. ° ′
11 12 15	5 49·51	35 55·9	80 41·1
30	51·57	44·9	13·9
45	53·72	33·6	79 45·5
13 0	55·95	21·8	15·8
15	58·27	9·6	78 44·7
30	6 0·70	34 56·9	12·0
45	3·26	43·6	77 37·3
14 0	5·99	29·5	0·2
	Atlantic Ocean.		
11 14 15	6 8·90	34 14·6	76 20·2
30	12·05	33 58·6	75 36·7
45	15·51	41·3	74 48·6
15 0	19·40	21·9	73 54·0
15	23·93	32 59·7	72 49·8
30	29·67	32·3	71 27·5
42	36·39	0·5	69 49·6
45	39·00	31 48·5	11·3
48·0	43·43	28·3	68 5·5
11 15 48·5	6 45·91	31 17·2	67 28·5

The Time of Beginning and End, &c. of the Eclipse of August 7, 1869, at four places where it will be Total.

United States, Boundary.

Lat. 49° 0′ 0″; Long. 107° 42′ 48″.

	Greenwich M.T. h m s	Boundary M.T. h m s
Beginning of the Eclipse	9 31 3·4	2 20 12·2 P.M.
,, Totality	10 38 47·1	3 27 55·9
Apparent Conj. of ☉ and ☽ in Longitude	10 40 31·6	3 29 40·4
End of Totality	42 15·7	31 24·5
,, the Eclipse	11 44 39·4	4 33 48·2
Duration of Totality	..	3 28·6
,, Eclipse	..	2 13 36·0
At Ap. Conj. Diff. App. Latitudes ☽ − ☉	..	☽ −0″·15
At Beg. and End of Totality, ☽'s D. − ☉'s D. = ☽ + 50″·79 and 50″·68		
Point of Beginning, Direct Vision, from ☉'s Vertex to the right hand		104°

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City of Des Moines, Iowa.

Lat. 41° 50′ 24″; Long. 93° 39′ 18″.

	Greenwich M.T.	Des Moines M.T.
	h m s	h m s
Beginning of the Eclipse	9 56 47·0	3 42 9·8 P.M.
,, Totality	10 59 23·0	4 44 45·8
Apparent Conj. of ⊙ and ☽ in Longitude	11 0 53·2	46 16·0
End of Totality	2 23·0	47 45·8
,, the Eclipse	59 32·5	5 44 55·3
Duration of Totality	..	3 0·0
,, Eclipse	..	2 2 45·5
At Ap. Conj. Diff. App. Latitudes ☽ − ⊙	..	☽ − 0″·03
At Beg. and End of Totality, ☽'s D. − ⊙'s D. =	☽ + 47″·73 and 47″·60	
Point of Beginning, Direct Vision, from ⊙'s Vertex to the right hand		120½°

Springfield, Illinois.

Lat. 39° 49′ 0″; Long. 89° 30′ 0″.

	Greenwich M.T.	Springfield M.T.
	h m s	h m s
Beginning of the Eclipse	10 3 10·4	4 5 10·4 P.M.
,, Totality	11 3 59·7	5 5 59·7
Apparent Conj. of ⊙ and ☽ in Longitude	5 25·2	7 25·2
End of Totality	6 50·5	8 50·5
,, the Eclipse	12 2 25·3	6 4 25·3
Duration of Totality	..	2 50·8
,, Eclipse	..	1 59 14·9
At Ap. Conj. Diff. App. Latitudes ☽ − ⊙	..	☽ − 0″·7
At Beg. and End of Totality, ☽'s D. − ⊙'s D. =	☽ + 46″·62 and 46″·49	
Point of Beginning, Direct Vision, from ⊙'s Vertex to the right hand		124°

Raleigh, North Carolina.

Lat. 35° 46′ 48″; Long. 78° 38′ 19″.

	Greenwich M.T.	Raleigh M.T.
	h m s	h m s
Beginning of the Eclipse	10 14 46·5	5 0.13·2 P.M.
,, Totality	11 11 20·2	56 46·9
Apparent Conj. of ⊙ and ☽ in Longitude	12 22·0	57 48·7
End of Totality	13 23·2	58 49·9
,, the Eclipse	12 5 27·6	6 50 54·3
Duration of Totality	..	2 3·0
,, Eclipse	..	1 50 41·1
At Ap. Conj. Diff. App. Latitudes ☽ − ⊙	..	☽ − 24″·34
At Beg. and End of Totality, ☽'s D. − ⊙'s D. =	☽ + 43″·57 and 43″·46	
Point of Beginning, Direct Vision, from ⊙'s Vertex to the right hand		130°

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The geographical position of Raleigh is supposed to be well known, but the assumed latitude of Des Moines and Springfield may be too great. A diminution of 10′ therein would make the computed times of the four contacts *later* at the former by 17·2, 19·2, 15·7, and 16·0 seconds, reduce the length of totality 3·5 seconds, and make the Ap. Lat. of the Moon − Sun's at conjunction + 5″·88, and at Springfield later by 16·8, 16·9, 15·4, and 14·0 seconds, reduce the length of totality to $2^m\,49^s\cdot3$, and increase the least distance of centres, Moon North, to 5″·70.

The width of the Moon's shadow in Iowa, Illinois, Indiana, &c., will be about 100 miles.

On the Corona round the Sun in Total Eclipes.

By Jos. Baxendell, Esq.

(*Extract from a Letter to Mr. Huggins.*)

I have been much interested in reading the report of a discussion which took place at the last Meeting of the Royal Astronomical Society, respecting the corona which is seen round the Sun during total eclipses. Recent observations and discoveries indicate very clearly that this corona is not an appendage of the Sun. What then is it? or how is its appearance to be accounted for? In a conversation I had some weeks ago with Professor Roscoe on the subject, I reminded him that in a paper which I read to the Literary and Philosophical Society in March, 1864, giving the results of a discussion of an immense number of magnetical and temperature observations made in different parts of the world, I showed that these results could be best explained by assuming the existence of an irregular nebulous ring circulating about the Sun, nearly in the plane of the ecliptic, and at a mean distance of 0·169; and I suggested to him that the reflexion of the Sun's light from the matter of this ring might be the real cause of the appearance of the corona in total solar eclipses. The view taken by M. Faye and the Astronomer Royal appears to me to be quite inadmissible. During that portion of the time of totality, when the corona is seen to the greatest advantage, no part of the Earth's atmosphere within a considerable angular distance of the Sun and Moon receives any direct sunlight, and, therefore, none can be reflected from it. Nor can I conceive how the corona can be due to any atmosphere about the Moon that could not be detected under other circumstances than those of a total solar eclipse. The differences in the appearances of the corona as seen and described by different observers cannot fairly be adduced as an argument against its cosmical origin; on the contrary, they are precisely what might be expected to occur with such an object seen under slightly different conditions of transparency of the atmosphere, and with eyes of different degrees of susceptibility to faint impressions of light.

Further investigations which I have made since I communi-

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cated the paper referred to above to the Literary and Philosophical Society have yielded no results that at all militate against the conclusions at which I had arrived.

Cheetham Hill, Manchester,
May 6, 1869.

On the Determination of the direction of the Meridian with a Russian Diagonal Transit Instrument. By Captain A. R. Clarke, R.E. (Abstract.)

The paper contains an account of the determination of the direction of the meridian at Findlay Seat, a mountain in Elginshire, 1104 feet high, 1868, Oct. 13 to 31, by means of a diagonal transit instrument,—that is, one in which the rays of light (instead of passing straight from the object-glass to the eye-piece) are bent at right angles by a prism in the central cube and so pass out at one of the pivots. The instrument used was made in Russia by M. Brauer, in the workshops of the Imperial Observatory at Pulkowa, and was lent by the Astronomer Royal to the Director of the Ordnance Survey; focal length of telescope about 31 inches, and aperture $2\frac{5}{8}$ inches; distance between the uprights 19 inches. In a copy of the instrument made by Messrs. Troughton and Simms, the telescope has 32 inches focal length, and $2\frac{6}{8}$ inches aperture. The general conclusion is, that considering the inherent difficulty of determining the direction of the meridian in so high a latitude, and the circumstances under which the observations were made,—in stormy weather, on the top of a high mountain, until the party was fairly driven away by snow and the destruction of some of the tents—the results furnish strong evidence in favour of the stability and excellence of the instrument. The observations and calculations were made in connexion with the Ordnance Survey, but were, by the permission of Col. Sir Henry James, presented to the Society. The observations at Findlay Seat were made by Quarter-master Steel and Sergeant Buckle, Royal Engineers.

Account of a series of Drawings of Mars.
By John Joynson, Esq.

The Opposition of *Mars*, which occurred on the 13th February last, has not been a favourable one for observations of the appearances on his disk; and, in addition to this, observers have had to contend with most unpropitious weather, the moist, open winter rendering our own atmosphere almost entirely unfit for the noting of the markings presented when the planet could be seen.

The accompanying drawings are selected from ninety, which

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I have been able to make at various times. They represent the planet during a complete rotation, and are intended as a sequel to those sent to the Society for the three previous Oppositions. They are so arranged that each diagram on the several sheets represents the same aspect of the planet, the appearance being, of course, altered somewhat at each opposition by the change in apparent position of the axis of the planet.

I have endeavoured, through the Society, to interest others in obtaining similar sets; but so far I am sorry to say that I have not met with much apparent success. It will be readily seen that there has been no absolute change in the Band, and the Channel from it to the North Pole, since the first set of diagrams were made in 1862. It is true that the set now sent appears to differ from that first taken, and it would be difficult at first sight to say that they were views of the same aspect of the planet; but those for 1864 and 1867 will sufficiently prove that to be the case, as the gradual alteration can by their aid be clearly seen. I have no doubt but this will continue during the future oppositions, and that it will be found that the same appearances will return in due course.

It has been observed at each opposition that the power of the eye-piece used, and also the presence or absence of hazy cloud, materially affected the colour of the disk; and I am disposed to think that the reddish look the planet sometimes had was chiefly, if not entirely, caused by our own atmosphere, as at these times even the reddest parts looked brownish, and sometimes yellow.

The colour of the Band and Channel has been dark, without any of the greenish appearance noted in former oppositions.

Waterloo, near Liverpool,
April 7, 1869.

Remarks on Mr. Joynson's Paper of Occultations.

By John J. Plummer, Esq., Observer at the Durham Observatory.

In the *Monthly Notices*, vol. xxix. No. 5, are some observations of Occultations by Mr. Joynson, observed near Liverpool, accompanied by some remarks, the object of which seems to be, to express a doubt as to the accuracy of the approximate times of these phenomena, given in the *Nautical Almanac*. Believing that the error was most probably on the part of Mr. Joynson, I computed for the three stars, *Aldebaran*, 119 *Tauri*, and 120 *Tauri*, the times of their disappearance and reappearance at Greenwich, with sufficient approximation; and it is perhaps superfluous to add, with results absolutely identical with those of the *Nautical Almanac*. I also computed by the same formulæ the times of the occultations of these stars for the latitude and longitude of Mr. Joynson's Observatory. The following are the results:—

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1869.			Calc. G.M.T. for Greenwich.	Angle from North Point.	Calc. G.M.T. for Liverpool.	Angle from North Point.	Obsd G.M.T. at Liverpool.
			h m	°	h m	°	h m s
Jan. 23	Aldebaran	Dis.	8 48·7	97	8 44·6	105	8 44 16
		Reap.	10 4·7	289	10 0·0	282	10 0 0
24	119 Tauri	Dis.	8 14·5	143	8 18·0	155	8 17 26
		Reap.	9 9·5	237	9 0·2	225	9 0 45
24	120 Tauri	Dis.	8 47·8	117	8 46·7	126	8 46 27
		Reap.	9 59·6	260	9 53·1	252	9 48 13

It will be seen that the agreement between the observed and computed times for Liverpool is sufficiently exact, with the exception of that for the last star, the observed time for the reappearance of which appears to be 5 minutes in error. The effect of the difference of parallax upon the observed time of the occultation of a star by the Moon, evidently, cannot be obtained without systematic calculation.

Permit me to call the attention of those astronomers who observe occultations at places remote from Greenwich, to a paper by the Rev. T. Chevallier, published in vol. xix. of the *Memoirs* of the Royal Astronomical Society, by which the effect of parallax on an occultation is readily calculable. I have observed during one year above twenty occultations, the approximate times of which have always been computed beforehand by tables founded upon that method. This computation is the work of less than two minutes, and the mean error in the approximate time thus obtained, I have found to be 34 seconds, a sufficient degree of accuracy, since the times in the *Nautical Almanac*, and upon which the computation depends, are given only to the nearest minute. What is perhaps of more importance, especially for observations of reappearances at the bright limb, is, that the approximate angle from the north point of the Moon is obtained with a mean error of $\frac{7}{10}$ of a degree only. Those who have computed the corrections due for the effect of parallax at different places by the long and rigorous formula, will appreciate the value of this short cut; and I shall be happy to compute tables upon the same plan as those I have myself used, for any Observatory where their value may be appreciated.

Durham Observatory, April 16th, 1869.

On the Period of η Argûs. By John Tebbutt, Jun.

(*Extract from a Letter to Sir J. F. W. Herschel, Bart.*)

"I have read with much interest the paper by Mr. Abbott on *η Argûs* and its surrounding nebula, at page 200 of the 28th

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vol. of the R. A. S. *Monthly Notices*, and your own remarks at page 225 of the same volume. You are already, doubtless, aware that I have carefully watched the variations in the light of this star during the past fifteen years. As it is not improbable that the star may soon begin to increase in lustre, I have watched it for the past twelve months, and am still watching it, with more than ordinary interest, in order to secure, if possible, the time and magnitude of the minimum. I have, therefore, on several occasions since April last carefully compared *η Argûs* with the principal stars in the Catalogue on page 42 of your 'Results of Astronomical Observations at the Cape of Good Hope from 1834–8.' In order to be assured of the identity of the comparison stars with the principal ones in your Catalogue, I determined their positions approximately by means of a telescope of 48 inches focal length and 3¼ inches aperture, roughly mounted as an equatoreal, a transit telescope of 2·1 inches aperture mounted on Y's secured to substantial piers of brick and stone, and a half-seconds chronometer. The comparisons with the telescope of 3¼ inches aperture were made with a ring-micrometer, whose diameter = 21′ 3″·6. The following Table gives the individual results of the comparisons, *η Argûs* being in each case employed as the comparison star:—

"*Co-ordinates with reference to η Argûs of certain Stars (Mags. 6½ to 7) in the Catalogue on page 42 of Sir J. Herschel's 'Results of Astronomical Observations at the Cape of Good Hope, 1834–8.'*

Date of Comps.	No. 1 or L. R.A.	No. 1 or L. Decl.	No. 2 or X. R.A.	No. 2 or X. Decl.	No. 522 or U. R.A.	No. 522 or U. Decl.
1868.	m s	′ ″	m s	′ ″	m s	′ ″
May 19	...	...	...	...	0 37·85 W.	18 24·9 S.
21	3 45·36 W.	17 29·9 S.	3 41·35 W.	0 12·3 N.	0 37·98 W.	18 30·4 S.
	...	...	...	...	0 38·86 W.	18 37·3 S.
	...	...	...	...	0 39·11 W.	18 35·6 S.
	...	...	...	...	0 38·61 W.	18 27·3 S.
22	3 45·62 W.	17 29·6 S.	3 42·35 W.	0 13·7 N.	...	...
Means	3 45·49 W.	17 29·8 S.	3 41·85 W.	0 13·0 N.	0 38·48 W.	18 31·1 S.

Date of Comps.	No. 1183 or W. R.A.	No. 1183 or W. Decl.	No. 1203 or Y. R.A.	No. 1203 or Y. Decl.
1868.	m s	′ ″	m s	′ ″
May 19	2 38·31 E.	...	2 58·99 E.	13 43·0 S.
21	2 38·38 E.	11 17·4 S.	2 58·30 E.	13 53·2 S.
	2 37·81 E.	11 10·5 S.	2 59·11 E.	13 50·1 S.
22	...	...	2 59·12 E.	...
Means	2 38·17 E.	11 14·0 S.	2 58·88 E.	13 48·8 S.

The letters N. S. E. and W. denote whether the star is north, south, east, or west of *η Argûs*.

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"By two comparisons of No. 387 with 1183, on the 22d May, I found that the former was $3^m\ 45^s{\cdot}50$ *west* and $14'\ 44''{\cdot}5$ *south* of the latter. Adopting $2^m\ 38^s{\cdot}17$ E. and $11'\ 14''{\cdot}0$ S. as the position of No. 1183, with reference to *η Argûs*, we have for the position of No. 387 or Z, with reference to *η Argûs*, as follows:—

R.A. = $1^m\ 7^s{\cdot}33$ *west.* Decl. = $25'\ 58''{\cdot}5$ *south.*

"I have also observed the stars with the transit instrument, but these observations give only the difference of R.A. The co-ordinates of the stars as I have given them agree with your Cape Results, precession &c. considered, within quantities which may be regarded as due to errors of observation rather than to changes in the relative positions of the stars.

"I trust the foregoing results, imperfect as they are, may be of interest to you, and I hardly need say you are at liberty to make what use of them you may think proper. Not being a Member of the Society, I do not receive the *Monthly Notices* as they are issued: hence the lateness of this communication.

"*Windsor, New South Wales,*
February 9th, 1869."

On the Period of η Argûs. By Prof. Elias Loomis.

In the *Astronomische Nachrichten*, vol. lx. p. 61, Prof. Wolf has endeavoured to show that the observations of *η Argûs* may be represented by a period of forty-six years, with one principal maximum and two secondary maxima in the intensity of its light. The observations recently published by Tebbutt (*Monthly Notices*, vol. xxviii. p. 266) indicate that this period is too short. I find that all the observations of the present century may be represented by supposing the period to be sixty-seven years; and if the curve representing the variations of brightness be twice repeated it will perfectly represent the two observations of 1677 and 1751. Future observations may require us to modify this conclusion; but at present it seems probable that *η Argûs* is a periodic star whose changes of brightness vary from the first to the sixth magnitude, and whose period is about seventy years.

The following table contains all the observations of this star with which I am acquainted, and the accompanying figure shows the curve which represents them.

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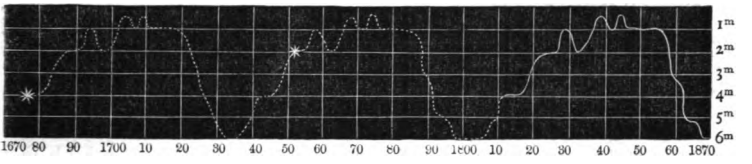
Date.	Mag.	Authority.	Date.	Mag.	Authority.
1677	4	Halley	1845	1	Jacob
1751	2	Lacaille	1850	1	Gilliss
1811	4	Burchell	1854	1	Tebbutt
1815	4	Burchell	1856	1·5	Powell
1822	2	Fallows	1859	3·1	Powell
1826	2	Brisbane	1861	3·6	Powell
1827	1	Burchell	1862	4·9	Tebbutt
1832	2	Johnson	1863	5·1	Tebbutt
1835	1·4	Herschel	1865	5·2	Tebbutt
1838	0·5	Herschel	1866	5·8	Tebbutt
1842	1	Maclear	1867	6	Tebbutt
1843	0·5	Maclear	1868	6	Tebbutt



If you consider this notice of sufficient importance you are at liberty to communicate it to the Astronomical Society.

Yale College, April 9, 1869.

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*Extract of a Letter from Dr. A Winnecke to E. J. Stone, Esq.**

"I have to announce to you (for communication to the Royal Astronomical Society) the discovery of a Comet, which I observed yesterday night:—

	h m			h m s	
April 9	12 24	Karlsruhe M.T.	α Comet	10 32 11	
			δ Comet	+ 33° 57′	

"This stranger will prove doubtless the expected Comet 1858 II., in the track of which it is situated. If I suppose the identity of the two bodies, the perihelion passage will take place June 25 or 26. It is faint, but large, 6′ perhaps 8′ in diameter.

"*Karlsruhe*, 1869, *April* 10."

* Previously issued as a circular, but inadvertently omitted from the last number of the *Monthly Notices*. Ed.

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Observations of Winnecke's Comet. By Hall Wortham, Esq.

1869.	Comet's R.A. h m s	Comet's N.D. ° ′ ″
April 29	10 3 0	36 32 0 (*a*)
May 4	9 57 0	36 42 0 (*b*)

Telescope employed, an achromatic of 6 inches aperture; power 50.
The above positions are *approximate* only.

(*a*). Comet distinctly seen as a faint nebulous patch of some little size, appearing occasionally to brighten somewhat to a centre, and followed at about its own diameter by a small, but brightish, star below the 9th magnitude. A larger star just beyond the upper part of the field. Telescope set by the Ephemeris by Dr. Winnecke (appearing in No. 1753 of *Astron. Nach.* p. 14) and comet found at once nearly in centre of field of view.

(*b*). Night not very clear, and the comet only just visible.

Royston, Herts, May 4th, 1869.

Observations of the same Comet. By S. J. Perry.

The nights following the 11th and 12th of this month were the only ones clear enough to enable us to observe the Comet which is approaching us at present. On May 11th, $14^h\ 7^m\ 13^s{\cdot}7$ Sid. T., the position was

R.A. $9^h\ 49^m\ 32^s{\cdot}7$ Decl. + 37° 4′ 10″

May 12th, $14^h\ 12^m\ 47^s{\cdot}16$,

R.A. $9^h\ 48^m\ 37^s{\cdot}16$ Decl. + 37° 3′ 45″.

These observations are uncorrected for refraction. If it would be of any use I could give you the positions for future fine nights, but I suppose there are so many good astronomers watching it at present that our observations will be of little use. There seems to be a slight condensation towards the centre, but no decided nucleus. This is all that our object-glass will give. I used a power of about 100, with full aperture 8 in.

I have just laid down a telegraphic wire between our two Observatories, which enables me to give the time and R.A. very correctly.

Stonyhurst College, Blackburn,
May 13, 1869.

Note on a Sun-spot seen March 14, 1869. By John Browning, Esq.

On the above date I was observing the Sun about 9.30 A.M.

Saw a large Sun-spot about 20° N. of the equator, and about 2′ from the E. limb.

Definition being very good I was able to use 12 inches aperture and achromatic power 148 and 208.

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To modify the light, I employed a *double*-prism solar eyepiece, and a corrected wedge of neutral tint glass.

The dimensions of the spot I found to be from N. to S. 14,400 miles, from W. to E. 19,600 miles. When comparing these measures with the drawing, it must be recollected that due allowance has been made in this calculation for the great foreshortening which occurs, owing to the spot being so near the edge of the limb.

The umbra contained three nuclei of very unequal dimensions, arranged nearly in the form of an equilateral triangle.

The drawing I made of the spot I have the honour to submit to the Society.*

Two bridges crossed the spot at an angle of about 40°. These bridges presented the appearance of broken twigs lying mostly in the direction of the bridges. It will be seen that both the bridges appear to be fed from a point to the N.E. of the spot.

The edge of the penumbræ furthest from the Sun's limb consisted almost entirely of bright faculæ. The extreme edge of the penumbræ was much darker than other portions, a point lying S.E. being the darkest of all.

This distinct darkening of the extreme edge of the penumbræ has often been doubted and ascribed to the effect of contrast; but if the spot were spirally formed, it would, I think, present some such appearance as I have described.

I have heard from Mr. G. P. Bidder, Jun., that on the afternoon of May 13, he saw a fine large spot, in which the bridges were spirally arranged. This observation I think very interesting.

During moments of the best definition the photosphere resembled broken branches, covered with long leaves, and overlying each other in all directions.

I would wish to speak with all diffidence on such a difficult matter of observation, but I cannot help thinking that the smaller granulations on the photosphere change considerably in appearance. I have seen them in the form of broken twigs, rice grains, and willow leaves, that is, sometimes blunt and sometimes sharp at the extremities, while at other times they were little more elongated than a common egg.

The very careful and interesting series of drawings taken by the Rev. F. Howlett show that Sun-spots have a tendency to assume regular forms. The cloudy matter which forms the photosphere, may frequently assume an appearance similar to that we may occasionally see in our own clouds, sometimes spoken of as "mackerel-back" clouds. If this be so it will be of little use to dispute as to whether these bodies are shaped like rice-grains or willow-leaves. We had better simply note their forms whenever we are so fortunate as to observe them distinctly.

* The drawing was exhibited at the Meeting. Ed.

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On a Remarkable Sun-spot observed May 1st, 1869.
By G. P. Bidder, Esq.

The accompanying drawing* represents the largest of a very fine group of spots, as it appeared upon the afternoon of Saturday, May 1st. It was chiefly remarkable for a distinctly apparent spiral arrangement, which was very striking. The whole of it gave the impression of an immense whorl, as shown in the drawing, which by no means exaggerates the resemblance.

The spot was traversed by an unusually attenuated bridge, which also appeared to participate in the spiral arrangement. The shortness of time during which the Sun was visible prevented my doing more than sketch the outline of the spot. One measurement was obtained which showed the penumbra to be in length about 27,000 miles. I examined the same spot again on the following day, when all traces of a spiral arrangement had entirely disappeared.

On a proposed Form for Spirit Levels. By Major Tennant.

My late employment has caused my thoughts to revert to some old projects for improving the spirit-level, and the subject is of much importance now that reliance is being daily more and more placed on the indications of these accessories. My original examination was, I think, in 1853, when I entered into an investigation of the properties a spirit-level should have on the supposition that the motion of the bubble was made by bodily transfer.

This hypothesis is widely removed from fact if we look to great movements of the level, but very nearly true for small ones,† and it leads me to a form of level and mode of construction which I have never seen elsewhere, and which I still think would produce results far superior to and far more certain than those of the present process.

I arrived at the conclusion which will, I think be received, that *the volume or contents of a bubble should be large, that its density relatively to that of the fluid should be small, and that its floor, or the surface common to the fluid and bubble, should have as large an area as possible,* and that these properties would be best gained by increasing the radius of curvature of the upper

* The drawing referred to was exhibited at the Meeting.

† If we make an alt-azimuth revolve about its vertical axis smartly, we see the bubble pass to the end of the tube, then to the other, and finally settle; and any great and sudden change of level causes a wave to pass along the floor of the bubble till equilibrium is established: but if only a small change be made by the foot-screw the bubble moves sensibly as one mass.

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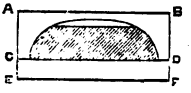
art of the level tube and making the fluid as heavy as possible compared with the bubble. In order that temperature should produce as little effect as possible on the length of the bubble, I would also reduce the volume of fluid, and to gain all these points I proposed to abandon altogether the cylindrical form of level tube and to cut out the necessary cavity from a rectangular prism of glass by means which should ensure that the longitudinal section of the cavity should be a segment of a circle and the transverse one uniform.

I proposed to take a prism, A B C D, of glass and to grind out of it a cavity of a somewhat elliptical shape transversely, as shown in the figure on the margin and in longitudinal section as below. C D E F is a flat plate cemented on to close the level.



Instead of filling the cavity with a volatile fluid of small density, whose vapour mixed with air forms the bubble, I think that the strength this new form might have would allow the use of mercury for the fluid and hydrogen perhaps (it would be necessary to exclude air) for the bubble. If mercury could be used it might be possible to float on it a plate of glass or iron carrying a mark, whose position could be referred to a scale with far more accuracy than the ends of the bubble can now be read; but I fear that this would be quite impracticable, and that the plate would have to be rejected from its liability to stick, if it fitted to the shape of the bubble floor closely enough to be useful.

At the time I have mentioned, I proposed, in a letter to the Surveyor General, to try making a level of this sort but difficulties intervened of various sorts, and it was laid aside. Any one probably would be able to devise the necessary apparatus for cutting the glass truly, but what I proposed was to have a cutting wheel revolving rapidly about a vertical axis and to traverse the glass plate before it. If it were rigidly fixed to two straight edges guided by cylindrical studs on a table, the longitudinal section would be a circle, and the form of the final cutting wheel would define the cross section.

It is hardly possible that I shall now be able to try this project (which I had nearly forgotten), but I have mentioned it to several persons, and I now offer it to the Astronomical Society in the hope that it may lead to an improvement in the mode of constructing levels.

Calcutta, September 25th, 1868.

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ERRATUM.

In Vol. XIX. of the Memoirs of the Royal Astronomical Society.

Page 234, line 13 from the top,—

for $B = \varrho_2 \cos \varphi_2 - \varrho_1 \cos \varphi_1$, *read* $B = \varrho_2 \sin \varphi_2 - \varrho_1 \sin \varphi_1$.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIX. *June* 11, 1869. No. 8.

Admiral MANNERS, President, in the Chair.

W. W. Magee, Esq., University College, London,

was balloted for and duly elected a Fellow of the Society.

Note on the Transit of Venus in 1874.

By J. R. Hind, Esq., F.R.S.

In the *Comptes Rendus* for 1861, July 22, will be found the results of a careful calculation by myself of the circumstances of the transits of 1874 and 1882 derived from M. Le Verrier's Tables of the Sun and Planet. Of the accuracy of the Right Ascensions and Declinations which I employed there can remain no doubt whatever; they have not only been confirmed by a new computation conducted in the *Nautical Almanac* Office, but the Astronomer Royal has since obtained identical results. I was, therefore, somewhat surprised to find that the times of contacts of the centre of *Venus* with the Sun's limb, given by M. Puiseux in the Appendix to the *Connaissance des Temps* for 1871, differed sensibly from the time I assigned in the *Comptes Rendus*, the same tables having been employed in both calculations. I had not much faith in the efficacy of a re-examination by myself of my own calculations, but Mr. Bishop having allowed me to place the matter in the hands of his assistant, Mr. W. Plummer, a very able and accurate computer, I am enabled to state that my published numbers are nearly correct, and that errors must have

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crept into M. Puiseux's work. The times deduced by Mr. Plummer are—

	G.M.T. h m s	Difference Plummer — Hind. s
Ingress.		
External contact, December 8th	13 46 42	− 14
Internal contact, ,,	14 15 30	− 27
Egress.		
Internal contact, December 8th	17 57 8	+ 3
External contact, ,,	18 26 6	+ 1

These differences for such a phenomenon are insignificant. The possible errors of any predictions of the times of contact must be very much larger.

1869, *June* 10.

The Transit of Venus in 1874. By R. A. Proctor, B.A.

In the *Monthly Notices* for March last, I indicated my intention of applying more exact modes of examination to the circumstances of the approaching transits of *Venus*, than had hitherto been considered necessary. In resuming the examination of the transit of 1874, I feel it necessary to call attention to that statement of my object, lest it should be thought that the corrections which I have been led to make on former results, point to errors in the processes by which those results were obtained, whereas, in reality, they are only due to the intentional neglect in prior work of considerations which even now many may be disposed to look upon as unnecessarily exact for the purposes of a preliminary inquiry. To still further obviate this objection (which, however, is not likely, I think, to occur to any one who has read my former papers), I shall for the most part refer to former results as those which have been obtained on such and such suppositions. This is the more advisable, because I find that astronomers are willing to accord the same degree of accuracy to my results in relation to the modes by which they have been obtained, as I am ready to assign to others results with a corresponding *proviso*.

It may be necessary, however, before proceeding to a detailed examination of the circumstances of the coming transit, to inquire (1) how far it is necessary or advisable to aim at exactness in the preliminary investigation of the subject; and (2) whether the phase which I have selected for examination is well chosen.

As a reason for applying a comparatively rough mode of

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examination to the circumstances of the transit, it has been urged that there is some uncertainty as to the exact moment of conjunction of *Venus* and the Sun, and as to the exact distance which will separate their centres. What is the range of possible error due to this cause I will leave to others to determine. My own impression is, that it is very small, whether considered absolutely or with reference to other sources of error which it is in our power to remove. But whether this be so or not, it seems clear to me that the possible errors, as to time and position-angle, arising from this cause, must be looked upon as affording additional reason for exactness in other matters. For, should any error resulting from disregard of exact considerations happen to be *additive* to errors resulting from inaccuracy in the Planetary Tables, it might happen that the conclusions to which we should be drawn would affect appreciably the success of observations to be made upon the transit.

The possible inaccuracy of the Planetary Tables seems to be the only source of error which it is out of our power to get rid of altogether. Therefore, as the choice of stations depends in certain cases on considerations of some nicety, it seems to me that the only basis for a proper selection is the determination, with as much accuracy as is readily attainable, of the circumstances under which the phase selected for observation will be seen at different stations. It must be remarked, however, that the formulæ and calculations by which I have determined these circumstances are not by any means necessary. A process which I will presently indicate suffices to give, with scarcely any labour, all the necessary information respecting a large number of stations, and that with a degree of accuracy amply sufficient for all the purposes of the inquiry. The only way, however, in which I could conveniently present my results for the study of those who take interest in the subject of this transit was that which I have selected.

As to the selection of internal contacts as the phase to be specially dealt with, there may be, of course, a variety of opinion. I am told, for instance, that in France there is some question of observing external contacts also. In this case, it would be advisable to make external contacts the determining phase (if a choice had to be made), or to determine for several stations the circumstances of both phases. For as at ingress external contact takes place about half an hour before internal contact (for every station), it is clear that the stations for observing retarded ingress (always so selected that the Sun is lately risen) must be chosen with reference to external contacts, if it is a *sine quâ non* that these are to be observed. If such a station were selected with reference to internal contact, with a moderately good solar elevation (necessarily *soon after* Sunrise), it is obvious that at the epoch of external contact half an hour earlier, the Sun would be too low; and considerations of the same sort apply to the observations of accelerated egress.

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It appears to me for these reasons that the consideration of both phases is inadmissible, and that though external contacts may be observed with advantage when they happen to be visible, *as well as* internal contacts, yet the stations must be chosen with strict reference to the latter phase, which is admittedly far the better of the two.

As M. Puiseux has given reasons for selecting the passage of *Venus' centre* over the Sun's limb as the phase to be considered in the preliminary inquiry, it may be well to examine how far those reasons are valid. He remarks that what is actually observed is the exterior or interior contact, but that, as the epochs of these phases are separated by nearly constant intervals for all stations, we may neglect the consideration of those intervals when all that we require is to determine the relative values of different stations. I venture to submit the following reasons for looking upon this line of argument as inadmissible:—

It is to be noted, in the first place, that the interval between external and internal contact is not constant, and although its range (from $26^m\ 24^s$ to $31^m\ 18^s$) may seem small, yet, where the values of stations run close, the differences thus resulting are quite sufficient to turn the scale in several instances. But also we may look at the matter in this light: the change of phase results in a change in the position of the stations of maximum acceleration, or retardation, and this change amounts in certain cases to hundreds of miles. Now the circles which give a definite coefficient of parallax in one of these cases, have equal radii, but their poles (the stations just mentioned) are separated by a considerable distance. Therefore the circles must intersect. At a point of intersection the coefficients are equal for either phase: on one side of that point, the nominal value of the coefficient is improved by the change of phase; on the other, it is deteriorated. This proves that the relative values of stations cannot remain unaffected by a change of phase, and suffices to account for the corrections introduced by my consideration of internal contacts in place of the passages of *Venus'* centre.

It would be incorrect, however, not to mention that the larger part of the corrections I now propose to consider, results from another cause than the change of phase; namely, from the effects of parallax,—which it has not hitherto been thought necessary to take into account. The application of the correction due to the equation of time has also to be mentioned as appreciably affecting the extent of the changes which are here dealt with.

I may summarise these changes under three heads:—

1. *The application of Delisle's method of absolute time differences.* The relative as well as the absolute values of many stations are affected by the change of phase. Some which had hitherto appeared unsuitable are found to be unobjectionable. Others which seemed good appear unfit. In other cases the relative values of two stations are so affected that the results of a comparison between them are directly reversed. Lastly, many

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stations not hitherto thought of in connexion with the transit are found to be well suited for the application of Delisle's method.

2. *The comparison between Delisle's and Halley's methods.* Halley's method is found, not merely to be applicable with advantage, which is all that can be said of it when central passages are considered, but to be superior to Delisle's,— slightly, when reference is made only to such stations as had been hitherto dealt with, noticeably when Antarctic stations are made use of.

3. *The comparison between the transits of* 1874 *and* 1882 *with reference to Halley's method.* This comparison, conducted according to the principles laid down by Mr. Stone (than whom no one is better entitled to pronounce authoritatively on such points) shows that Halley's mode may be applied much more advantageously to the transit of 1874 than to that of 1882.

I premise that I have carefully gone over all the calculations on which the results of my former paper were founded, and have applied so many independent modes of calculation that I can no longer entertain any doubt as to the accuracy of those results.*

The results to be now brought into comparison for the sake of forming an estimate of the effect of phase, parallax, and the equation of time, upon the values of various stations, may be thus classed:—

A, those derived from the consideration of central passages, as supposed to be seen from the Earth's centre, with the position-angles corresponding to external contact.

B, those derived from the same phase, similarly seen with the position-angles corresponding to central passage.

C, those derived from the consideration of internal contacts, as seen from the stations themselves, and with the position-angles corresponding to the phase so seen.

Note.—The results under head C have alone been corrected for equation of time.

The following table exhibits the position-angles and epochs (for ingress) corresponding to A, B, and C:—

	Position-Angles.			Epochs.		
	A	B	C	A	B	C
	°	° ′	° ′	h m	h m	h m
Accelerated Ingress	131	133 29	133 56	14 0	13 55	14 4
Mean Ingress	131	133 29	136 28	14 0	13 55	14 16
Retarded Ingress	131	133 29	139 29	14 0	13 55	14 29

In preparing the columns under head C in the accompanying

* This paper had originally exhibited an independent mode of establishing my conclusions, founded on a simple construction applied to the elements given in Mr. De La Rue's paper on the transit (*Monthly Notices* for December 1868). For the sake of brevity, I have thought it best to omit this part of the paper, the accuracy of my conclusions not being, I find, called in question. The mode of construction is, however, briefly referred to further on.

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tables, I have made use of six orthographic maps which accompany this paper. These were constructed with every precaution to insure accuracy. The intersection of longitude-lines and latitude-parallels (to every 10°) were separately constructed for by a double process, and in all critical cases further tests were applied. In all, the construction of the maps involved upwards of 3000 measurements, and from the scale on which the maps were constructed, the practice I have had in orthographic mapping, and the care used in the present instance to insure accuracy, I feel that I may safely claim for the numbers under the columns headed C in the following tables, a satisfactory degree of trustworthiness.

The six maps include four quarter-spheres, exhibiting the solar elevations and the coefficients of parallax in the same way as in the maps 1-4, in the *Monthly Notices* for December last, though in a different projection. The other two exhibit the Earth as supposed to be seen from the Sun at ingress and egress (*mean*, and for internal contacts). In these the solar elevations are indicated by circles, and in place of 10 parallactic parallels, corresponding to the parallactic circles in the other map, there are laid down parallactic lines corresponding to minute intervals (the line across the earth's centre being taken as a zero-line). These lines are not parallel, but separately constructed for. Thus their indications differ somewhat from those derived from the parallactic circles in the other maps, which are laid down on the supposition (not strictly correct) that the outline of the penumbra of *Venus* travels parallel to itself across the face of the earth. This will account for a slight want of correspondence between the second and third C-columns in the following tables: the third gives the correct effect of parallax. It will be noticed, however, that the difference is always trifling in the case of places suitable for the application of Delisle's method.

A further correction, but one of small importance, would result from the consideration that the apparent outline (supposed to be seen from the Sun) of that part of *Venus*' penumbra which traverses the Earth is not a straight line, but part of a large circle. Thus the actual outline on the Earth's surface is not part of a circle. It follows that the parallactic curves in the four quarter spheres ought not to be circular, and the parallactic lines across the other two maps ought to be curved, the direction of their curvature being turned in the direction towards which the shadow is moving for ingress, and the reverse for egress. All the corrections due to this cause are minute, and attain their greatest values at places not suitable as stations either for the application of Delisle's or Halley's method.

A correction has been applied to columns C corresponding to the fact that the maps are severally constructed for a single epoch, while the events to which they relate occupy several minutes.

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Table I. *Accelerated Ingress.*

Station.	Sun's Elevation.			Coefficient of Parallax.		Acceleration in Minutes.	
	A	B	C	A	C	B	C
	°	°	°			m	m
Woahoo	22·5	..	19·8	·92	·93	..	11·2
Hawaii	22·3	21·5	19·7	·92	·92	10·3	11·1
Aiton I. (Aleutian)	12·0	..	10·8	·80	·84	..	10·3
Marquesas I.	20·0	23·1	17·7	·71	·66	7·5	7·9
Mouth of Amoor R.	15·0	..	14·0	·57	·62	..	7·6
Jeddo	..	30·9	32·1	..	·50	..	6·8
Otaheite	34·5	34·3	29·7	·59	·54	6·1	6·4
Nertchinsk	..	..	10·1	..	·41	..	5·8
Tsitsikar	..	..	17·0	..	·43	..	5·8
Kirin Oula	..	..	19·5	..	·42	..	5·7
Nagasaki	..	..	32·7	..	·40	..	5·3
Tientsin	..	..	22·2	..	·38	..	5·0
Pekin	..	20·2	20·8	..	·30	3·0	4·3
Shanghai	..	29·5	28·5	..	·25	2·6	3·9
Nankin	..	..	27·1	..	·20	..	3·6
Canton	..	..	35·5	..	·08	..	1·6
Hongkong	..	..	36·2	..	·08	..	1·6

Table II. *Retarded Ingress.*

Station.	Sun's Elevation.			Coefficient of Parallax.		Acceleration in Minutes.	
	A	B	C	A	C	B	C
	°	°	°			m	m
Crozet I.	9·5	..	15·0	·98	·96	..	12·6
Enderby Ld.	..	17·3	20·0	..	·92	10·3	11·8
Kerguelen Ld.	25·0	23·6	27·5	·91	·88	10·3	11·6
Macdonald I.	..	27·2	31·0	..	·85	..	11·2
Kemp I.	..	..	30·0	..	·85	..	11·1
Bourbon I.	4·5	..	12·4	·93	·84	..	11·1
Mauritius	6·0	..	14·1	·92	·81	..	10·7
Amsterdam I.	..	27·6	34·1	..	·77	9·8	10·3
Rodriguez	11·5	..	19·0	·89	·76	..	9·9
Sabrina Ld.	..	..	45·0	..	·73	..	8·2
Adelie Ld.	..	..	45·0	..	·50	..	6·8
S. Victoria Ld.	..	36·4	38·5	..	·46	5·0	6·0
Perth (Aust.)	..	..	65·0	..	·38	..	5·3
Royal Co. I.	..	..	62·0	..	·32	..	4·5
Madras	12·5	..	21·0	·47	·25	..	4·0
Bombay	4·5	..	12·5	·44	·22	..	3·8
Macquarie Ld.	..	..	52·0	..	·25	2·9	3·5
Hobart Town	..	70·0	67·0	..	·20	..	2·8
Adelaide	..	..	75·0	..	·18	..	2·5
Melbourne	..	..	75·0	..	·13	..	2·2

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Table III. *Accelerated Egress.*

Station.	Sun's Elevation.			Coefficient of Parallax.		Acceleration in Minutes.	
	A	B	C	A	C	B	C
	°	°	°			m	m
South Victoria Ld. (Possession I.)	..	23·8	25·0	..	·89	10·3	11·4
Adelie Ld.	..	..	34·0	..	·83	..	10·6
Campbell I.	..	..	26·0	..	·79	..	10·3
Emerald I.	..	..	30·0	..	·78	..	10·3
Macquarie I.	24·0	..	32·0	·83	·75	..	9·8
Chatham I.	11·5	..	16·0	·87	·75	..	9·8
Canterbury (N.Z.)	19·0	..	22·5	·81	·71	..	9·3
Wellington	17·0	..	20·0	·81	·70	..	9·2
Sabrina Ld.	..	..	43·0	..	·70	..	9·2
Enderby Ld.	..	41·0	39·0	..	·65	6·9	8·5
Royal Co. I.	..	..	42·0	..	·64	..	8·5
Auckland	15·0	14·4	19·2	·78	·64	8·6	8·5
Kemp I.	..	..	51·0	..	·57	..	7·6
Hobart Town	36·0	36·0	40·0	·66	·57	7·5	7·6
Melbourne	38·0	..	43·0	·58	·48	..	6·6
Sydney	33·0	..	37·2	·59	·48	..	6·6
Adelaide	..	..	47·8	..	·40	..	5·8
Kerguelen Ld.	..	60·5	57·1	..	·40	..	5·0
Crozet I.	..	..	47·5	..	·34	..	4·2
Perth (Aust.)	..	..	66·2	..	·25	..	3 6

Table IV. *Retarded Egress.*

Station.	Sun's Elevation.			Coefficient of Parallax.		Retardation in Minutes.	
	A	B	C	A	C	B	C
	°	°	°			m	m
Orsk	12·0	..	12·5	·98	·98	..	11·8
Omsk	12·5	..	11·5	·96	·97	..	11·7
Astracan	12·4	..	12·0	·97	·97	..	11·6
Aleppo	14·2	..	14·6	·91	·89	..	10·5
Peshawur	..	..	31·5	..	·85	..	10·3
Alexandria	13·0	..	14·0	·86	·84	..	10·0
Suez	16·0	17·6	16·1	·85	·83	9·4	9·8
Nertchinsk	..	..	10·1	..	·81	..	9·8
Delhi	..	..	38·0	..	·78	..	9·4
Tsitsikar	..	..	12·0	..	·72	..	8·7
Bombay	..	..	45·0	..	·70	..	8·5
Pekin	..	21·2	21·0	..	·70	7·7	8·6
Kirin-Oula	..	..	14·0	..	·69	..	8·4
Tien-tsin	..	..	17·1	..	·68	..	8·4
Calcutta	..	..	45·3	..	·68	..	8·2

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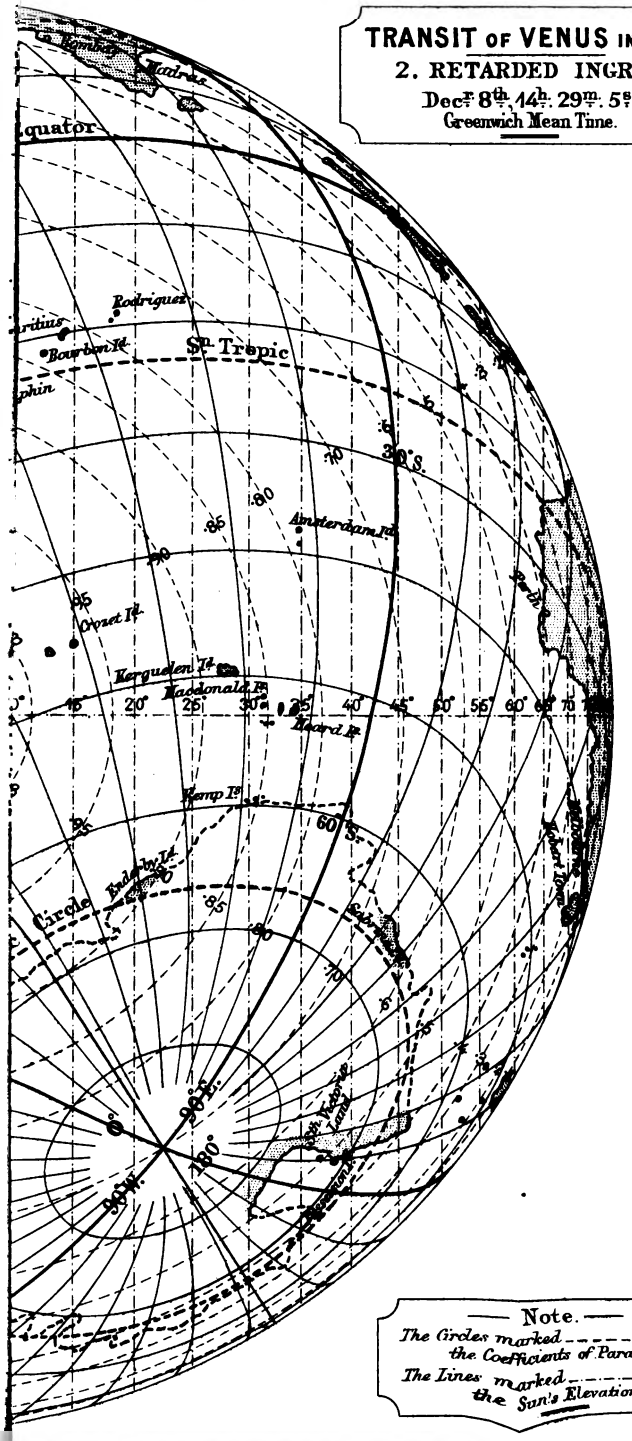

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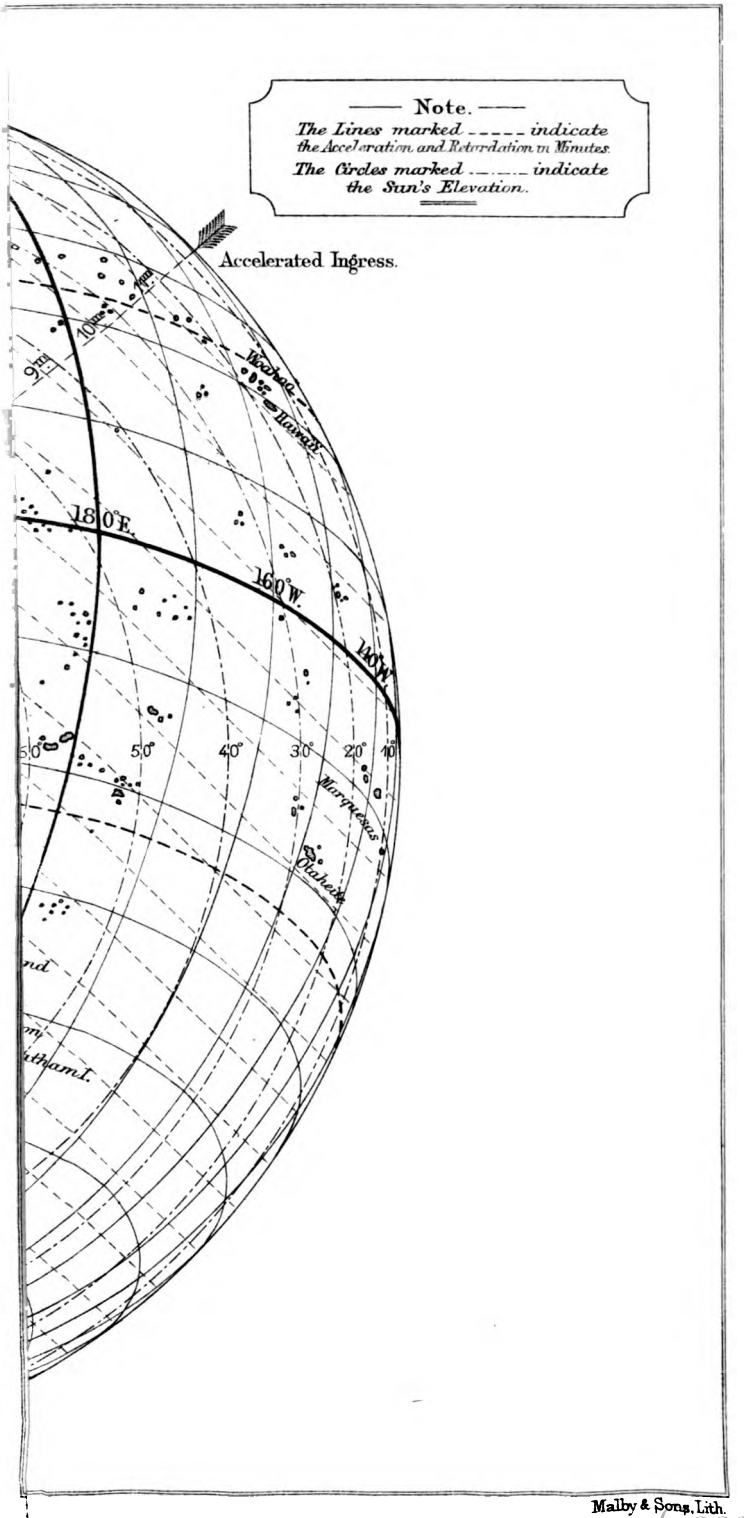

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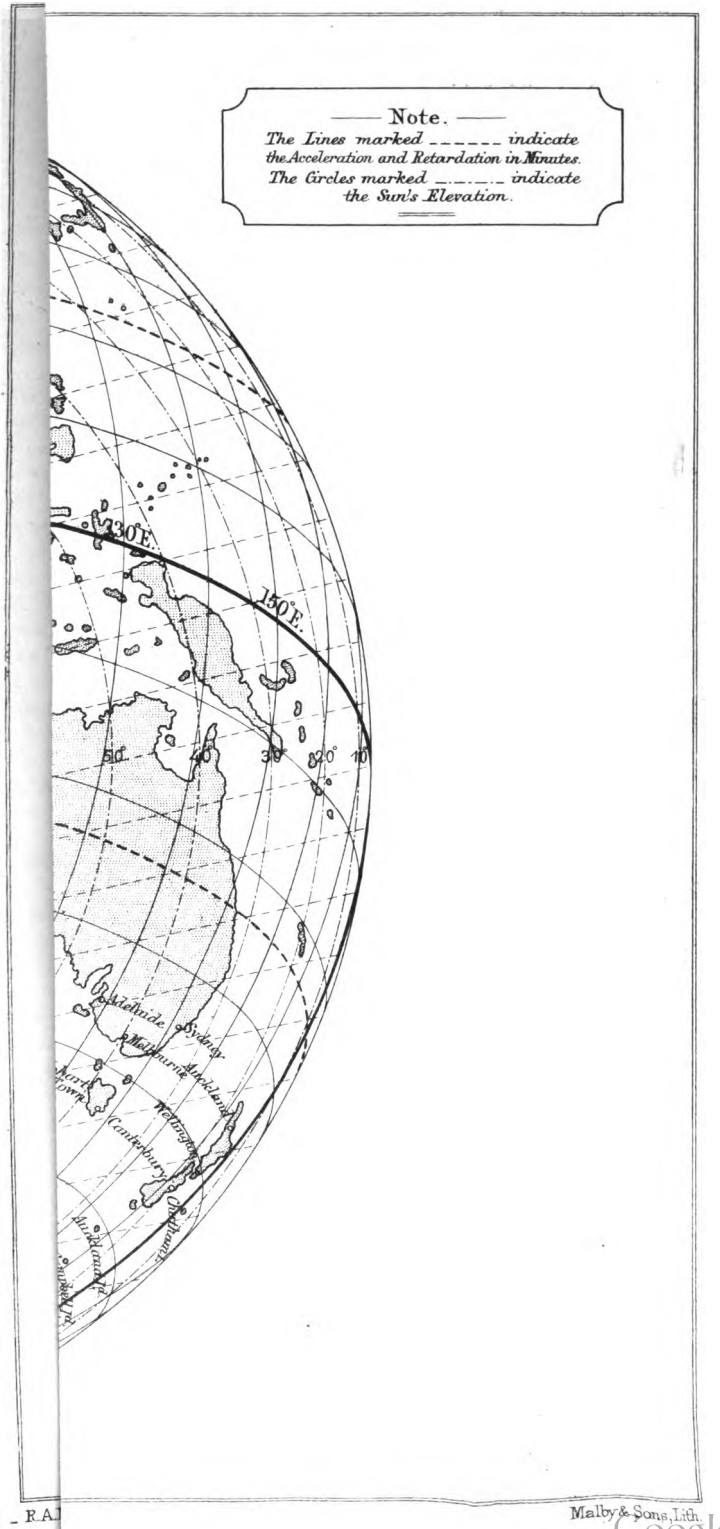

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Station.	Sun's Elevation.			Coefficient of Parallax.		Retardation in Minutes.	
	A	B	C	A	C	B	C
	°	°	°			m	m
Aden	..	..	36·0	..	·66	..	7·8
Nankin	..	..	27·0	..	·62	..	7·6
Madras	..	..	52·0	..	·61	..	7·4
Shanghai	..	26·1	26·0	..	·57	6·2	7·2
Canton	..	..	37·0	..	·52	..	6·6
Hongkong	..	..	37·0	..	·50	..	6·5

It will be seen, on a comparison of tables A, B, and C, that the effects of the change of phase are in some cases important. The coefficients of parallax are affected in several instances by more than 0·1, and in two cases by 0·22. In the cases of Crozet Island (Table II.) and Chatham Island (Table III.) solar elevations are so improved, that these stations, which would have to be rejected if central passage were considered, are shown to be well-suited for the observation of internal contacts. The diminution of all the coefficients in Table III., through the change of phase, has an important influence on the value of Delisle's method, so far as egress observations are concerned. It is important to notice, also, that under heads C in Tables III. and IV. many stations not hitherto recognised as available are included among the best places for observing egress. The Indian stations in Table IV. seem too valuable to be neglected. Peshawur is better even than Alexandria; Delhi is not inferior to the latter station (when solar elevation is considered as well as coefficient of parallax). Bombay, Calcutta, and Madras are also excellent. It may be noticed also that Bombay and Madras, which, when considered with reference to central passage, had seemed suitable places for the observation of retarded ingress, are found to have so poor a coefficient of parallax when reference is made to internal contacts, that it would be useless to observe ingress there (so far at least as the application of Delisle's method is concerned).

Of course, it will be impracticable for this country to send observers to more than a certain number of stations. But it is not unlikely that besides Russia, France, and England (the only countries specially concerned in the transit of 1874), other nations may care to take part in the solution of the noble problem of determining the Sun's distance; and thus it seems advisable that all the stations where there will be any chance of obtaining useful observations, should be tabulated as nearly as possible according to their relative values.*

* It may not be amiss to point out that the numbers tabulated under head C in the above tables may be readily tested in a few minutes for any assigned station. Indeed, the whole work of determining the circumstances of the transit for internal contacts, as seen from any station, may be gone through in about an hour, and the main part of the work thus gone through would not require to be repeated for other stations. Although I have no reason to doubt the accuracy of the results tabulated above, it may be well that they should be checked.

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As regards the comparison between Delisle's and Halley's method for the transit of 1874, I may remark that M. Puiseux's results seem to have been somewhat misinterpreted. He does not anywhere speak of Halley's method as the best, but simply states that he can see no reason why it should not be applied: nor do his figures establish the superiority of Halley's method.

I believe I shall be able to show, however, that there is at least a possibility that Halley's method may be so applied to the transit of 1874, as to give absolutely the best means of deter-

The justice of the following processes will be obvious to every one acquainted with the principles on which the transits of an inferior planet depend:—

Take out from the *Nautical Almanac* and tables of the planetary elements,—

App. Semi-diameter of Sun on December 8 ..	= D
,, ,, Venus in inf. conj. ..	= V
Horizontal Parallax of Venus (in inf. conj.) refd. to Sun	= P
Sun's Southern Declination on December 8 ..	= δ
Equation of Time	= ϵ

On any convenient scale describe concentric circles with radii

(i.) D (ii.) D + V (iii.) D − V

(iv.) D − V + P (v.) D − V − P

On circle (ii.) take (according to Mr. Hind's elements of the transit) position-angles 130°·7 from South towards East, and 160° from South towards West, and join the points thus determined. Determine with a protractor the position-angles Q_1, Q_2, Q_3, and Q_4, for the intersections of this chord with the circles (iv.) and (v). Again, making the extremities of the chord correspond to hours $13^h\ 47^m$ and $18^h\ 26^m$ (from Mr. Hind's elements of the transit) divide the chord into hour-divisions. Thence determine epochs H_1, H_2, H_3, H_4, corresponding to the intersections of the chord with the circles (iv.) and (v.).

Now adjust a globe so that, looked at from above (vertically), its appearance shall be that presented by the earth, supposed to be seen from the Sun, at apparent solar time $H_1 + \epsilon$, and with southerly solar declination δ. From the southern intersection of the brazen meridian with the horizon-circle, take an arc of Q_2 towards the East along the latter circle. This will give the place where ingress is most accelerated. To determine the value of any station near this point, measure the arc-distance of the station from the point (α), and from the horizon-circle (β). Then cos α = coefficient of parallax, and β = Sun's elevation. And thus the coefficients and solar parallax may be determined for any number of stations, with corresponding methods for retarded ingress and for egress.

It may be necessary to notice that, to convert coefficients of parallax into minutes of acceleration or retardation, reference must be made to the intervals of maximum acceleration and retardation. These are, for accelerated ingress and retarded egress (Tables I. and IV. respectively) $11^m\ 58^s$; for retarded ingress and accelerated egress (Tables II. and III. respectively) $13^m\ 8^s$. A coefficient of parallax p corresponds for the former pair of cases to a time-interval

$$[11^m\ 58^s - 35^s\ (1 - p)]\ p,$$

for the latter to a time-interval

$$[13^m\ 8^s + 35^s\ (1 - p)]\ p.$$

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mining the Sun's distance available before the transit of 2004 (about the circumstances of which I know nothing). It only requires that the same energy should be devoted (either by England or some other nation) to the coming transit, which has been called for in relation to the transit of 1882. There will also be the same chance of failure in one case as in the other.

To prove the justice of these views I point to Nertchinsk (or its neighbourhood) as a suitable station for observing the lengthened transit, and the neighbourhood of Enderby Land as a proper station for observing the shortened transit. The above Tables give for the former station a lengthening of $(5^m{\cdot}7 + 9^m{\cdot}8)$ or $15^m{\cdot}5$; and for the latter a shortening of $(11^m{\cdot}8 + 8^m{\cdot}5)$ or $20^m{\cdot}3$. The total difference of duration is thus shown to be $35^m{\cdot}8$. Setting against this the best case for Delisle's method (Woahoo from Table I. and Crozet Island from Table II.), we get the total difference of absolute time, is $(11^m{\cdot}2 + 12^m{\cdot}6)$ or $23^m{\cdot}8$. And the relative values of the methods (in these cases the most favourable for each) are given by the formula

$$\frac{\text{Halley's}}{\text{Delisle's}} = \frac{(35{\cdot}8)^2}{(23{\cdot}8)^2} \frac{\{(4{\cdot}28)^2 + (1)^2\}}{(4{\cdot}28)^2}*$$

$$= \frac{2477}{2075}\dagger \text{ approximately;}$$

and in this ratio, independently of all the other advantages which it presents, does Halley's method surpass Delisle's. By taking Kemp Island, Adelie Land, Victoria Land, Crozet Island, or Kerguelen's Land for the southern, and Tsitsikar, Kirin-Oula, or Tien-tsin, for the northern stations, we get differences of duration ranging from 33^m to $30^m{\cdot}6$, corresponding (according to Mr. Stone's formula) to a range from 24^m to $22^m{\cdot}2$, in the case of Delisle's method. There is little chance of the latter method being applicable with a difference greater than the lowest of these methods; and the highest is a difference which the best stations will not give by Delisle's method.

When it is remembered that Halley's method is so much the simpler, and that stations selected with reference to it give a double chance of at least a useful observation, the above considerations seem to decide the question of the relative values of the two methods in 1874.

* Here I have used the values given in Mr. Stone's paper (*Monthly Notices*, April 8, pp. 251, 252). He would have obtained a larger value for e had he calculated for internal contacts. This would, of course, have been a change unfavourable to my case. But against this I set the fact that his careful examination of the circumstances of internal contacts will tend to render the probable errors much smaller (relatively) in 1874 than they were in 1769, when the observers had very vague notions of the phenomena they were to expect and to watch.

† It would not be correct to reduce the number of minutes 35·8 in a certain proportion, and take the excess over $23^m{\cdot}8$, as measuring the superiority of Halley's mode. The proper way of treating the question is to indicate the relation between the two modes by a ratio, as above.

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Lastly, as to the relative values of the transits of 1874 and 1882, considered with reference to Halley's method.

Mr. Stone's remark, that the choice of stations is limited to those at which the Sun will have an elevation of at least 10°, reduces the maximum available difference of duration in 1874 from $36\frac{3}{4}^m$ (the value I had before mentioned) to $35\frac{3}{4}^m$. But the transit of 1882 is much more seriously affected. The suggested station near Sabrina Land must be rejected at once. And, although the suggested station near South Victoria Land corresponds to an elevation of 10° at ingress (see map 5, *Monthly Notices* for December last), there is no accessible spot in that neighbourhood which will give any such elevation. At Possession Id. the Sun's elevation will not be much more than 5° at ingress; and at Coulman Island, the most southerly station which Antarctic seamen hope to reach, the Sun's elevation will be but 7°; and even if these islands were suitable, they give a difference of duration perceptibly less than that which I had dealt with in my former paper.

I must add that I had fully taken into account the difference in the clinging of the disk of *Venus* to the Sun's limb in 1874 and 1882. Indeed, I had adopted a considerably greater proportion than that indicated by Mr. Stone. But, as the principle he lays down requires that both Sabrina Ld. and Victoria Ld. should be dismissed from our consideration in 1882,—and as there is absolutely no other southern station at all comparable with these two, as far as the lengthening of the transit's duration is concerned, we seem forced to the conclusion that if Halley's method fails totally for either of the coming transits, it is for that of 1882 and not for that of 1874.

On the general question of the value of the transit of 1874, I find myself compelled to adopt a different opinion from that which Mr. Stone has expressed. Although the comparative slowness of ingress and egress increases the probable error, it increases the available time-differences in at least the same proportion,—I say, *at least*, because I cannot believe that the errors of an observation of ingress or egress increase fully in the proportion of the slowness with which the phenomena succeed each other. But even assuming this unfavourable view, it may yet be shown that, *cæteris paribus*, slow transits (*i. e.* transits of short chord) are as valuable as more rapid ones when Halley's method is considered, and more valuable when Delisle's method is considered. For, in the first case, we have

(1), Value of a transit varies as maximum observable difference;

(2), Maximum observable difference varies as slowness of rate of ingress or egress* (*cæteris paribus*);

(3), Value of a transit varies as $\frac{1}{\text{Slowness of rate of ingress}}$;

* Estimated, of course, perpendicularly to the limb.

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therefore, for transits of different rates, but having circumstances otherwise equal, the value is appreciably constant, so long as the chord of transit is not too near a diameter of the Sun.

For the second case we have (1) and (2) unaltered, but in place of (3) we get the

$$\text{Relative value of transit varies as } \frac{1}{\left(\frac{k^2}{v^2}\right) + l^2};$$

where k is some constant, v represents the rate of ingress or egress, and l is the error in determining longitude of a station. Thus, the value of a transit varies as $\frac{1}{v^2} + \left(\frac{k^2}{v^2} + l^2\right)$ or inversely as $(k^2 + v^2 l^2)$, and the slower transits are therefore the most favourable.

It is worthy of notice, however, that, *as a rule*, when there are two transits separated by eight years, the first is the less favourable for the application of Halley's method. The present case shows that to this rule there are exceptions.

Field Notes on the Zenith Telescope. By G. Davidson, Esq.

(*Communicated by Prof. Piazzi Smyth.* Abstract.)

The principle of the method of determining the latitude with the Zenith Telescope is based upon the proposition that when the meridional zenith distances of the two stars at their upper culmination (one being north and one south of the zenith of a station) are equal, the colatitude is the mean of their north polar distances.

When the zenith distances of the pair of stars differ by an arc less than that subtended in the field of view of the telescope, that arc is measured by means of the micrometer attached to the telescope; and, at the times of observation, the error in direction of the vertical axis of the instrument in the plane of the meridian is measured by a delicate level.

The micrometer and level are therefore essential means by which the principle of the method is reduced to a practical field application: whilst the generally unsatisfactory correction for refraction in observations depending on the measure of simple zenith distances is avoided by applying the differences of refraction for the small difference of zenith distance of the two stars.

The paper contains a full discussion (illustrated by examples) of the methods of ascertaining the instrumental errors and other corrections as used in the U. S. Coast Survey; and of two different methods used for reducing the observations; and it is shown how the instrument may be used for extra-meridian observations for latitude, &c., which observations, by means of known stars too far apart for the application of the preceding accurate methods, may (it is remarked) be often used with advantage on a geographical reconnaissance or for some special object.

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Note on Lambert's Theorem for Elliptic Motion.

By Prof. Cayley.

Consider any two positions, A, B, in an elliptic orbit, focus S, and semi-axis major $= a$; then if ϱ, ϱ', c denote the radius vectors S A, S B and the chord A B respectively, and if $P, = \frac{2\pi a^{\frac{3}{2}}}{\sqrt{\mu}}$, be the periodic time, the time of passage from A to B is given by the formula

$$\text{Time A B} = \frac{P}{2\pi}(\chi - \chi' - \sin\chi + \sin\chi')$$

where

$$2a\cos\chi = 2a - \varrho - \varrho' - c, \quad 2a\cos\chi' = 2a - \varrho - \varrho' + c.$$

To fix the ideas we may consider the time of passage as being in every case positive; and, for Time A B, the motion from A as being towards the apocentre; Time B A will, of course, in like manner denote that the motion from B is towards the apocentre; and we thus have according to the positions of A, B, either Time A B = Time B A; or else Time A B + Time B A = P.

This being so (see the *Theoria Motus*, p. 120), χ will be always a positive arc between 0 and 360°; χ' a positive or negative arc between 0 and ± 180°; and moreover χ' will be positive or negative according as the described focal angle is $< 180°$ or $> 180°$; whence, cos χ' being known, the arc χ' is determined without ambiguity.

But as noticed in the place referred to, there is when only ϱ, ϱ', c, a, are known, a real ambiguity as regards the arc χ; viz. χ may be either the arc $> 180°$ or the arc $< 180°$, having for its cosine the given value of cos χ. For, given the points S, A, B, and the semi-axis major a, there exist two elliptic orbits determined by these data; and the two values of χ correspond to the times of passage between A and B, in these two orbits respectively. If, however, the actual orbit be given, there is no longer any real ambiguity; and it must be possible to decide between the two values of χ: the criterion is, in fact, a very simple one, viz. drawing a chord from A through the other focus H of the ellipse, this either separates, or it does not separate, B from the force-focus S; and I say that in the expression of Time A B, in the former case (viz. when chord A is a separator) we have $\chi < 180°$; in the latter case (viz. when chord A is not a separator) we have $\chi > 180°$.

It of course follows that, in the case of transition, when the line A B passes through H, we must have $\chi = 180°$: this is at once seen to be so; for $\chi = 180°$ gives the condition $4a = \varrho + \varrho' + c$; but if σ, σ', are the distances of A B from the focus H, then $2a = \varrho + \sigma$, $2a = \varrho' + \sigma'$, and the condition becomes $\sigma + \sigma' = c$; that is A B must pass through H.

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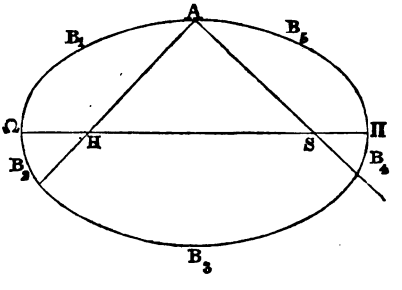
As a verification of the new criterion, I consider the point A as having a fixed position on the orbit, but the point B as having successively different positions; and writing down the two formulæ

$$\text{Time A B} = \chi - \chi' - \sin\chi + \sin\chi',$$
$$\text{Time B A} = \omega - \omega' - \sin\omega + \sin\omega',$$

(where for simplicity the constant factor $P \div 2\pi$ is omitted) I proceed to compare these for different positions of the point B. We have, in every case, $\cos\omega = \cos\chi$, and $\cos\omega' = \cos\chi'$; whence (χ, ω being each positive and less than 360°) $\omega = \chi$ or else $\omega + \chi = 360°$, viz. the former equation subsists if ω, χ, are each less or each greater than 180°, the latter if the one is greater, the other less than 180°. And again (χ', ω' being each less than $\pm$ 180°) we have $\omega' = \chi'$, or else $\omega' = -\chi'$, according as ω', χ' have the same or opposite signs.



Now in the figure, suppose that B occupies successively the different positions B_1, $B_2 \ldots B_5$, the criteria for χ, χ' (or ω, ω') give as follows,

	Ch. A.	Ch. B.	∴			∠ A S	∠ B S A.		
1	sep.	not	$\chi < 180°$	$\omega > 180°$	or $\omega + \chi = 2\pi$	$< 180°$	$> 180°$	$\chi' = +$	$\omega' = -$ or $\omega' = -\chi'$
2	sep.	sep.	$<$	$<$	$\omega = \chi$	$<$	$<$	$+$	$+$,, $\omega' = \chi'$
3	not	not	$>$	$>$	$\omega = \chi$	$>$	$>$	$+$	$+$,, $\omega' = \chi'$
4	not	not	$>$	$>$	$\omega = \chi$	$>$	$>$	$-$	$-$,, $\omega' = \chi'$
5	not	sep.	$>$	$<$	$\omega + \chi = 2\pi$	$>$	$<$	$-$	$-$,, $\omega' = -\chi'$

Hence substituting for ω, ω' their values in terms of χ, χ', we have

$$\begin{aligned}
\text{Time A } B_{1\ldots5} &= \chi - \chi' - \sin\chi + \sin\chi' \\
\text{,, B } A_1 &= 2\pi - \chi + \chi' + \sin\chi - \sin\chi' \\
\text{,, B } A_2 &= \chi - \chi' - \sin\chi + \sin\chi' \\
\text{,, B } A_3 &= \chi - \chi' - \sin\chi + \sin\chi' \\
\text{,, B } A_4 &= \chi - \chi' - \sin\chi + \sin\chi' \\
\text{,, B } A_4 &= 2\pi - \chi + \chi' + \sin\chi - \sin\chi'
\end{aligned}$$

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and thence (restoring the omitted factor $P \div 2\pi$)

$$\begin{aligned}
\text{Time A B}_1 &+ \text{Time B A}_1 = P,\\
\text{,, A B}_2 &- \text{,, B A}_2 = 0,\\
\text{,, A D}_3 &- \text{,, B A}_3 = 0,\\
\text{,, A B}_4 &- \text{,, B A}_4 = 0,\\
\text{,, A B}_5 &+ \text{,, B A}_5 = P,
\end{aligned}$$

which are the relations which in fact subsist between the times $A B_1$ and $B A_1$ &c.

On the Nature of the Appearances on Sun-spots, called Bridges of Light. By Professor E. W. Brayley.

At the last meeting of the Society I suggested as desirable an inquiry whether some of the appearances, termed *Bridges*, of which the drawings, exhibited by Mr. Bidder and Mr. Browning, of the Sun-spots observed and described by them,* presented remarkable instances, were not, in reality *faculæ* over the spots—of the nature of the floating faculæ described and photographed by Mr. De La Rue—viewed in a direction perpendicular to the Sun's surface, or in that of the geometrical axis of a spot.

My opinion that such would be found to be the fact eventually was supported by a statement made by Mr. Huggins, that he had observed *Bridges* exceeding in brightness the average brightness of the photosphere.†

I have since found that in my third paper on the Physical Constitution of the Sun, in the *Companion to the Almanac* (for 1866, p. 77) the following observations on the phenomena in question are cited from the *Solar Physics* of the Kew observers; being a portion of their inductive argument that a spot is a phenomenon which takes place beneath the level of the brighter part of the Sun's photosphere.

"There are a good many instances in which a spot breaks up in the following manner. A bridge of luminous matter of the same apparent luminosity as the surrounding photosphere, and unaccompanied by any penumbra, appears to cross over the umbra, or centre, of a spot. There is good reason to think that this bridge is really above the spot; for were the umbra an opaque cloud, and the penumbra a semi-opaque cloud, both being above the Sun's photosphere [as supposed by Kirchhoff], it is unlikely that the spot would break up in such a manner that the terrestrial observer should not perceive some penumbra accompanying the luminosity. Again, detached portions of luminous

* *Monthly Notices* for May 14, pp. 300–302.

† See *Astronomical Register* for June, 1869, pp. 124, 125.

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matter appear to move across a spot without producing any permanent alteration."*

These observations are in entire harmony with my opinion that these bridges are portions of faculæ, and with the confirmatory observations of Mr. Huggins and Mr. Browning, of the great brightness of some bridges.

It is requisite that I should here remark that I confine this interpretation to the phenomena for which I adopt the original designation by Sir J. F. W. Herschel, of "*bridges of light;*"† not extending it to the other "bridges" seen to connect the opposite sides of a spot, the constitution of which is manifestly the same with that of the penumbræ, and which have every appearance of being at the same level with them. This discrimination is in agreement with the inferences of the Kew observers already stated.

From the structure of the two bridges on the spot observed by Mr. Browning on March 14, which, as described by him, was evidently the characteristic structure of penumbræ, while yet the bridges, it is said, were the brightest parts of the object represented, it is probable that in his observation the two kinds of bridges were optically confounded together, on account of the partial superposition, in this instance, of bridges of light over penumbral bridges.‡

* *Solar Physics*, First Series, p. 31, sect. 27. In a note on the fact last described I have suggested that these detached portions of luminous matter are, perhaps, some of the hypothetical bodies which I have termed *meteoritic masses*. After considering, however, the observations, both telescopical and spectroscopical, since made of the solar disk, I no longer retain this idea.

The term "meteoritic masses" is nearly equivalent to that of *meteoroids*, used by Professor H. A. Newton to designate the bodies which become sensible to us as meteors, but which, until recently, were otherwise utterly unknown, though their existence was certain, and had been the subject of several reasonable hypotheses. Now, however, we have grounds for believing, according to the judgment of astronomers in general, that they are related to comets, and, in my own opinion, that they have also a genetic relation to the Sun. See *Companion* for 1866, pp. 91–95, and for 1868, pp. 125–126; also, *Proceedings of the Royal Society* (vol. xiv. pp. 120–129) for March, 1865, and *Report of the British Association for the Advancement of Science* for that year, Appendix to Report of Committee on Luminous Meteors, pp. 130–131, and 140–142.

† See *Results of Astronomical Observations made at the Cape of Good Hope;* "Observations of the Solar Spots," p. 435 (par. 428). The accompanying plate (xvii.) presents, on a small scale, in figs. 4, 6, 7, 13, characteristic curved, if not spiral examples, of these objects.

‡ Graphical representations of celestial objects should, of course, describe with all possible fidelity what the observer believes that he sees, leaving the due interpretation of the several appearances which make up the visible projection for future scrutiny. But it would appear to be desirable that telescopic observers of the Sun should now endeavour to discriminate between the two kinds of "bridges," and to ascertain the respective limits of each, where they are not superposed. In many figures of solar configurations both appear to be represented, but as if they were continuous portions of the same physical objects, differing only in brightness.

In observing the Sun in the profound manner, so to speak, which, in the actual condition of heliography and solar physics, astronomers are called upon to adopt, "the combination of the photographic record" of the telescopic pro-

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The varying forms of the bridges of light, observed—always within the penumbral area, and commonly at its inner edge bordering the umbra, but sometimes crossing the latter,—on the remarkable spot seen in October, 1865, as described and figured by the Rev. F. Howlett,* are also strictly conformable with the view of their nature now taken. They are such as would be presented by the extended vertices of a spiral or curved sheet or volume of flame or incandescent gaseous matter, rising from the area of the umbra. We see in them, in all probability, the upper termination (as a physical object, neglecting at present the consideration of its chemical nature) of the vorticose or cyclonic torrent, of which, as viewed in horizontal section, the umbra of the spot is merely the visual projection. In all probability, again, such torrents are liquid below and aeriform above, a constitution which would account for some apparent inconsistencies in the recorded spectroscopical observations of the several physical elements of spots. The spectra of bright lines seen over umbræ, both by Mr. Huggins and Mr. Lockyer, would naturally be caused by their upper gaseous portions; a subject to which I have alluded in the *Monthly Notices* for January.

I intended now to discuss, as then intimated, the mutual relations of Sun-spots, faculæ, and prominences, considered as the results of one continued action, beginning in the nucleus of the Sun, including the compatibility of that induction with the local distribution of the spots; and also the character of the markings by which the faculæ-prominences may be expected to be recognised in photograms of the solar surface viewed in the stereoscope. But I must reserve both subjects for the next session of the Society.

I may remark, however, that the recent spectroscopical observations of Mr. Lockyer† and Padre Secchi,‡ assuming their accuracy, are conclusive as to the identity of faculæ and prominences as portions of the same physical objects, while they are in perfect accordance, also, with the theory of the constitution of

jection with "the stereoscopic examination of it" will be invaluable. I formerly remarked that, "Admitting the powers of" this method of research "to be correctly estimated by Mr. De La Rue, and assuming that there is no source of fallacy in its use, it may be doubted whether any other means at present known to science will so adequately avail for the discovery of the order of superposition of the several parts or objects of which a given area of the visible photosphere consists, or their arrangement in altitude vertical to the Sun's surface."—*Companion*, 1864, p. 40.

Mr. De La Rue's observations of the floating faculæ, which I have discussed in the *Monthly Notices* for January, and those of the nearly related phenomenon of the comparative elevation of bridges of light, cited from him and the other Kew observers in the present paper, are accordant examples of the value of this combined method in astronomical research, a crucial addition to which is now afforded by the spectroscope.

* *Monthly Notices*, November, 1865, pp. 12–20, figs. 5 to 10.

† *Proceedings of the Royal Society* (vol. xvii. pp. 415–418) for April 29, 1869.

‡ *Comptes Rendus* (vol. lxviii.), May 10, 1869, p. 1082. The entire contents, however, of P. Secchi's two papers here published bear immediately on the objects of this reference.

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the spots which I announced in a paper, entitled "Inferences and Suggestions in Cosmical and Geological Philosophy," inserted in the *Proceedings of the Royal Society* for March, 1865. This would require that the spectrum of the umbra of a spot should not exhibit any line of absorption which is not present in that of the Sun's normal surface, and also that no ordinary solar line should be wanting in it; facts actually observed by Mr. Huggins;* with whose results, in this respect, those obtained by his contemporaries are in agreement.

London Institution, June 10, 1869.

On Solar Currents. By C. H. Weston, Esq.

(*Extracts from Note-Book.*)

1869, June 4th. Large spot changed since the 2nd. Penumbra with increased margin, especially on S. shore of umbra.—5th June. A large luminiferous promontory running out from E. side of S. shore with extremity expanded. Shut off Sun's disk by diaphragms. Now the expanded or club end was seen bifurcated, and of the two branches the W. one longest. In afternoon W. branch more elongated, and also with branchlet at right angles to the branch. Towards end of branchlet two little buddings like incipient transverse arms of a Roman cross.

One end of this luminous promontory was attached to the penumbra, and not to the overlying photosphere. The margin of the penumbra was so broad on the S.E. side that not only was the connexion apparent, but the penumbra itself visibly intervening between the penumbral promontory and photosphere. Thus, while the S.E. extremity was undoubtedly penumbral (physically and chromatically) the other was as brilliant as the photosphere itself and the terminal point especially so; preponderating evidence that the N.W. end was in the act of being lifted up into the upper regions of the photosphere.

6th June. Same branch and branchlet increased in breadth and now pretty uniformly broad, and reaching to W. margin. Perceived well by strong contrast of subjacent umbra, that the promontory was unquestionably *arched* on the S.E. side, while the N.W. end appeared to touch the photosphere and partook of the same brilliancy; further confirming the idea that the part of S.E. penumbra was arched up (from rising currents) with its N.W. end on the *higher* photospheric elevation.

7th June. Examined another spot. No penumbra apparent on the S.E. side. Faculous matter (clearly part of the photosphere)

* See *Phil. Trans.* 1868, p. 553.

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extended across the umbra. On N.W. side the penumbra was visible on each side of the jutting cloud. Towards N.W. the extremity was dichotomized by a black line (recalling *Saturn's* ring) evidently showing umbra beneath. Three hours later the cloud lengthened, but throughout the entire length same lustre at photosphere, showing an approximate horizontal movement from S.E. to N.W.

Another spot examined, showing a bridge truly penumbral, extending from E. to W. margin of umbra. In the afternoon the *bridge more brilliant*, and so also the *inner margin of the penumbra* (in contact with the umbra), while the external margin was not so illuminated,—evidence of upward movement both in the bridge and peripheral parts of penumbra bordering on the umbra.

These three cases seem to show locally-upward and horizontal currents.*

I have given these descriptions as concisely as possible, and will not, therefore, add any remarks on luminosity. But I must confess that I concur with M. Faye in thinking there is no necessary connexion between the degree of photospheric luminosity and solar caloric.

Ensleigh Observatory,
Lansdowne Bath, June 1869.

Probable Errors of Greenwich Observations in Zenith Distance, estimated by mere discordances from the separate means. By E. J. Stone, Esq.

No.	Z.D.	P.E.	No.	Z.D.	P.E.
	°	″		°	″
1	0	0·47	14	44	0·59
2	26	0·48	15	45	0·60
3	28	0·49	16	46	0·61
4	30	0·50	17	47	0·62
5	32	0·51	18	48	0·63
6	34	0·52	19	49	0·64
7	36	0·53	20	50	0·65
8	38	0·54	21	51	0·66
9	39	0·54	22	52	0·67
10	40	0·55	23	53	0·68
11	41	0·56	24	54	0·69
12	42	0·57	25	55	0·70
13	43	0·58	26	56	0·71

* Used (full apertures) two Newtonian Equatoreals, 9·0 and 14·15 inches, and 8·75 and 16·5 feet respectively.

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No.	Z. D.	P. E.	No.	Z. D.	P. E.
	°	′		°	′
27	57	0·72	42	72	0·87
28	58	0·73	43	73	0·89
29	59	0·74	44	74	0·92
30	60	0·75	45	75	0·95
31	61	0·76	46	76	0·98
32	62	0·77	47	77	1·02
33	63	0·78	48	78	1·08
34	64	0·79	49	79	1·14
35	65	0·80	50	80	1·20
36	66	0·81	51	81	1·28
37	67	0·82	52	82	1·38
38	68	0·83	53	83	1·52
39	69	0·84	54	84	1·68
40	70	0·85	55	85	1·92
41	71	0·86	56	86	2·29
			57	87	3·32

The number of observations employed in obtaining these probable errors is more than 2000.

The probable errors were first determined for ten principal points, and a curve was traced amongst these points: the results given are those read off from the curve. Observations of *Sirius* and *Rigel*, which are incessantly observed at Greenwich, and observations of which are often made under unfavourable circumstances and in the daytime, have rather larger probable errors than the general average, and have not been included in the results.

Occultations of Stars by the Moon. By John Joynson, Esq.

I have not had an earlier opportunity of seeing Mr. Plummer's paper remarking on one of mine on "Occultations of Stars by the Moon," in vol. xxix. No. 5.

I am afraid that I have given Mr. Plummer a considerable amount of trouble; but my apology, if he considers one needful, must be that I found that sometimes, when the disappearances and reappearances occurred at or about the same angle, there was a greater difference than seemed to be accounted for by the parallax.

I am glad, however, to find that I was mistaken, and that the calculated times agreed so well with the observations I sent to the Society, with the exception of that of the reappearance of 120 *Tauri*.

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Occultations of Stars by the Moon.

1869.			G.M.T. h	m	s	
19 Feb.	48 Tauri	Disappearance	9	4	40·3	cloudy at reappearance.
23	ζ' Cancri	,,	10	11	40·2	,,
24	π^3 Cancri	,,	10	45	16·9	
		Reappearance	11	43	3·5	
22 March	*g* Geminorum	Disappearance	9	17	16·7	
		Reappearance	10	26	21·8	
16 April	119 Tauri	Disappearance	7	29	44·2	
		Reappearance	8	23	19·8	
	120 Tauri	Disappearance	8	18	25·6	
		Reappearance	8	52	48·5	
18 May	Regulus	Disappearance	9	54	27·9	
		Reappearance	10	7	25·2	

Latitude 53° 28′ 24″ N.
Longitude $0^h\ 12^m\ 7^s$ W.

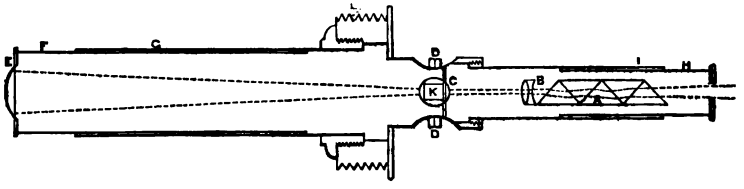
Waterloo, near Liverpool,
3 June, 1869.

On a Simple Form of Star Spectroscope.

By John Browning, F.R.A.S.

A simple, efficient, and economical spectroscope for Astronomical work has long seemed to be a desideratum.

The instrument which I have the honour to exhibit and describe to the Society will, I think, be found better adapted for use with telescopes of moderate aperture than any other contrivance hitherto proposed.



In the diagram, A is a compound direct-vision prism consisting of five prisms, B is an achromatic lens which focusses on the slit C, by means of a sliding-tube H, both the prisms and lens are fastened in this tube; K is a small right-angled prism covering half the slit, by the aid of which light may be seen reflected through the circular aperture in front of it.

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In this manner a comparison may be made with the spectra of metals or gases. The reflecting prism, with the ring to which it is attached, can be instantly removed and the whole length of the slit used if desired. D D is a ring milled on the edge, on turning this round both edges of the slit are made to recede from each other equally, being acted on by two hollow excentrics. The lines can thus be increased in breadth without their centres being changed, a point of importance. E is a cylindrical lens attached to the tube F, which slides in another tube G.

To use the spectroscope the adapter L, which is the same size as the Society's thread, has only to be screwed into the eye-drawer of the telescope in the place of the ordinary Huyghenian eye-piece.

The drawer must then be adjusted so that the slit G comes exactly to the focus of the object-glass. This should be tried beforehand, by getting an image of the Sun and then making a mark on the drawer-tube of the telescope. When this has been done the tube can be set by this mark, and the spectroscope screwed into it at any time without any trouble in adjustment.

If the cylindrical lens be removed the spectrum of a star will be a mere line of light.

The cylindrical lens is for the purpose of widening this line to such an extent that the lines in the spectrum may readily be discerned. For this purpose the lens must be placed *with its axis at right angles with the slit*, and the best distance from the slit will be between three and six inches. The nearer it is brought to the slit the broader will be the spectrum, but it should not be used too close, on account of the diminution of the light.

When it is desired to obtain the spectra of planets, comets, or nebulæ, or indeed any heavenly bodies possessing considerable diameter in the telescope, the cylindrical lens may be dispensed with advantageously.

The weight of the instrument, as I have figured and described it, being only about seven ounces, much less than an ordinary micrometer, it will not seriously affect the balance of a telescope.

Various portions of this apparatus have been for some time used by different observers. The arrangement of the whole in combination with the new slit is the only novelty in the contrivance.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIX. *Supplementary Notice.* No. 9.

On the Cluster in Perseus. By Dr. O. Pihl.

In the Supplementary Number of the *Monthly Notices* for 1868 was inserted a paper of mine, on the examination of one of the clusters in *Perseus*.

In preparing for publication a detailed account of the subject, I have detected an error in one of the coefficients employed for the reduction of the declinations of the stars, which has caused a systematic error in that co-ordinate, making it too great in some cases by about 1″·5.

Since the paper in question was published, the number of observations has been increased for the declinations of a few stars, which offer peculiar difficulties of observation, and some slight modification in the mean of their declination has, therefore, been obtained.

The following list gives the corrected *relative* values in declination for 85 stars measured, as referred to the star No. 25 in the group:—

No.		No.		No.	
1	+ 759·77	8	+ 816·73	15	+ 1394·77
2	+ 830·24	9	+ 1119·64	16	+ 472·04
3	+ 948·71	10	+ 1380·16	17	+ 1458·96
4	+ 432·36	11	+ 258·30	18	+ 1021·76
5	+ 1250·69	12	+ 300·01	19	+ 806·23
6	+ 354·56	13	+ 1442·02	20	+ 1378·18
7	+ 995·04	14	+ 1387·26	21	— 177·88

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No.		No.		No.	
22	= + 805″·59	44	= + 577″·88	65	= + 390″·80
23	+ 895·22	45	+ 700·07	66	+ 319·27
24	− 120·94	46	+ 555·66	67	− 34·93
25	·00	47	+ 384·94	68	+ 480·22
26	+ 1483·48	48	+ 668·70	69	+ 77·33
27	+ 1028·90	49	+ 104·51	70	− 95·94
28	+ 403·40	50	+ 987·12	71	+ 820·11
29	+ 642·24	51	+ 1039·42	72	+ 1096·72
30	+ 440·39	52	+ 1035·67	73	+ 1515·15
31	+ 639·40	53	+ 34·78	74	− 53·93
32	+ 962·33	54	+ 605·16	75	+ 54·65
33	+ 1490·75	55	+ 29·96	76	+ 1304·00
34	+ 287·51	56	+ 598·93	77	+ 930·17
35	+ 708·87	57	+ 34·30	78	+ 719·72
36	+ 716·84	58	+ 1370·13	79	+ 485·43
37	+ 223·61	59	+ 561·17	80	+ 659·27
38	+ 560·53	60	− 1·03	81	+ 742·46
39	− 277·01	61	+ 1158·77	82	+ 114·32
40	− 165·15	62	+ 859·01	83	+ 642·28
41	+ 413·08	63	+ 839·50	84	+ 671·64
42	+ 623·75	64	= + 824·87	85	= + 722·34
43	= + 864·40				

Tullinbjerget, Christiania, 26th July, 1869.

On the Comparative Clinging of the Limb of Venus to that of the Sun in the Transit of 1874 *as compared with that of* 1882. By R. A. Proctor, B.A.

Although Mr. Stone's paper on the above subject in the April Number of the *Notices*, and my own remarks to the same effect in the March Number, perhaps indicate with sufficient exactness the relation between the two transits in the respect dealt with, it may be worth while to determine the actual intervals which will elapse between real and apparent internal contacts for both transits.

I shall adopt Mr. Stone's estimate that about $\frac{1}{60}$th part of the diameter of *Venus* may be allowed for the effects of irradiation (*Monthly Notices* for November, 1868).

Venus in transit (at her rising node) appears to traverse a space equal to her own diameter in $15^m\ 16^s{\cdot}88$, and therefore $\frac{1}{60}$th of her diameter in $15^s{\cdot}28$. This would be in the interval between real and apparent contacts in the case of a central transit.

Now, in 1874 (see p. 216 of my paper in the March Number of the *Monthly Notices*) the chord traversed by *Venus*, from internal

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contact most accelerated at ingress to the same phase most retarded at egress, subtends at the Sun's centre

$$360° - (133° 56' + 163° 27') = 62° 37'.$$

Hence the angle at which *Venus's* apparent path is inclined to the Sun's limb, at the epochs of these phases is 31° 18′; and the interval between real and apparent internal contacts is therefore

$$= 15^s{\cdot}28 \times \operatorname{cosec}(31° 18')$$
$$= 29^s{\cdot}41.$$

In a similar manner the corresponding interval for internal contact most retarded at ingress, and for the same phase most accelerated at egress

$$= 15^s{\cdot}28 \times \operatorname{cosec}(25° 48')$$
$$= 35^s{\cdot}11.$$

The difference, by the way, is greater than one could expect, and shows how necessary it is, in all such cases, to test our views by calculation.

For 1882, I use the position-angles given in a Note in the present Number. I find the interval between real and apparent contacts, for internal contact most accelerated at ingress and the same phase most retarded at egress

$$= 15^s{\cdot}28 \times \operatorname{cosec}(48°{\cdot}3)$$
$$= 20^s{\cdot}46;$$

while for internal contact most retarded at ingress and most accelerated at egress, I find the interval between real and apparent contacts

$$= 15^s{\cdot}28 \times \operatorname{cosec}(45°{\cdot}5)$$
$$= 21^s{\cdot}39.$$

We see, at once, from these values that a given difference of duration will be much more valuable in 1882 than in 1874. Whether an observer in 1874, having from $29^s{\cdot}41$ to $35^s{\cdot}11$ during which to watch the separation of *Venus* from the Sun's limb would make as large a *proportional* error as an observer in 1882 who has an interval of from $20^s{\cdot}46$ to $21^s{\cdot}39$ during which to watch the same phenomenon, may be questioned; that he would make a larger *absolute* error is certain. Assuming the former view we may proceed to compare the time-differences which would have equal values in 1874 and in 1882. Let P be the ratio in which a difference in 1874 must exceed one in 1882, that the two may have equal values; then we must have

$$P^2 = \frac{(29{\cdot}41)^2 + (35{\cdot}11)^2}{(20{\cdot}46)^2 + (21{\cdot}39)^2}$$

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(for Delisle's method assumed to be applied to the greatest advantage in either case,—but the ratio is approximately true also for Halley's method). It follows that

$$P = 1{\cdot}547.$$

The ratio I had made use of in the March Number of the *Monthly Notices*, was $50\frac{1}{3} : 32\frac{3}{4}$, which reduces to 1·533, and is appreciably correct; nor does Mr. Stone's value, 1740 : 1219 (or 1·427) in the April Number, differ importantly from the true ratio.* It is worthy of notice, however, how readily a change of the phase considered affects all such proportions. One would have thought that the value which Mr. Stone obtained by considering the intervals between external and internal contact, could not differ appreciably from that which I had obtained by considering the intervals between internal contacts most accelerated and the same phase most retarded by parallax; yet the change is about as from 14 to 13.

Sept. 11th, 1869.

Note on the Transit of Venus in 1882. By R. A. Proctor, B.A.

The elements marked (*) in the following table have been deduced from the formulæ discussed in my paper in the March Number of the *Notices*; the others are Mr. Hind's fundamental elements of the transit:—

	G.M.T. h m s	Position-Angle. From S. to E. °
First External Contact (from Earth's centre)	1 55 38	34·7
*First Internal Contact (most accelerated by parallax)	2 7 51	32·6
„ „ (from Earth's centre)	2 15 56	..
* „ „ (most retarded by parallax)	2 24 18	29·8
		From S. to W.
*Last Internal Contact (most accelerated by parallax)	7 44 5	61·3
„ „ (from Earth's centre)	7 52 27	..
* „ „ (most retarded by parallax)	8 0 32	64·1
Last External Contact (from Earth's centre)	8 12 47	66·1

In the Paper referred to above I expressed my belief "that the corrections which have to be made in the case of the transit

* There is a misprint at p. 221, of the March Number of the *Monthly Notices*, line 13 from the bottom of the page; instead of "observable difference" "maximum difference" should be read. The printer probably took the former reading from the previous sentence. The context shows plainly what was meant; because I immediately apply the maximum differences ($50\frac{1}{3}^m$ in 1874 and $32\frac{3}{4}^m$ in 1882) to the purpose considered. This error, which I have only just detected, serves to explain what had hitherto been quite unintelligible to me, the fact, namely, that Mr. Stone should have written the Paper on the clinging of *Venus* to the Sun's limb without noticing that I had already fully considered the effects of that peculiarity.

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of 1882" (when internal contacts are considered instead of passages of *Venus's* centre across the Sun's limb) "are less considerable than those" I had made in the case of the transit of 1874. This view is justified by the calculated positions of the places where ingress and egress are most affected by parallax. These I find to be as follows:—

		Lat.	Long.
(i.)	The place where first internal contact is most accelerated, lies in	51° 5′ S.	86° 48′ E.
(ii.)	The place where first internal contact is most retarded, lies in	53 16 N.	94 28 W.
(iii.)	The place where last internal contact is most accelerated, lies in	26 18 N.	40 1 W.
(iv.)	The place where last internal contact is most retarded, lies in	23 46 S.	137 11 E.

Not one of these places differs by much more than 300 miles from the corresponding place obtained by considering the passage of *Venus's* centre, with the position-angle for external contact,—the phase with reference to which these places had hitherto been calculated.

The transit of 1882 has an interest which that of 1874 wants, in being partially visible in England.

September 11, 1869.

Note on Atmospheric Chromatic Dispersion as affecting Telescopic Observation, and on the Mode of Correcting it. By George Biddell Airy, Astronomer Royal.

In my last communication to the Royal Astronomical Society on the Transits of *Venus*, I adverted to the injurious effect, on the observations, which might possibly arise from the chromatic dispersion produced by the atmosphere. I had carefully observed *Mercury* on the Sun's disk, and I was painfully struck with the colour and consequent indistinctness of the upper and lower limbs both of the Sun and of *Mercury*, and with the prospect of a total loss, in the observations of the Transits of *Venus*, of that delicacy on which the success of the observations will greatly depend: (it being remarked that, for the most valuable observations, the Sun must be low and the atmospheric refraction and dispersion must therefore be considerable; and that the planet must be near the highest or lowest part of the Sun's limb). And I suggested that probably an efficient corrective might be found, in the application of a glass prism of small refracting angle in the eye-piece of the telescope. It is my purpose now to give the numerical elements for the preparation of the necessary prism, and to state the success which attends its use.

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The optical theory is exceedingly simple, but it may conduce to clearness that it be given here. Let θ be the angle of incidence when a ray of light falls on a refracting surface; $\theta + \psi$ the angle of refraction; then ψ is the angle of deviation. Also, if the refractive index be $1 + m$, m may be called the index of deviation; m is negative for the passage of light from vacuum to air, and for the passage from air to glass, but positive for the passage from glass to air. At each of these refractions we have the accurate equation,

$$\sin(\theta + \psi) = (1 + m) \sin \theta;$$

and, if θ remain invariable, but m be varied, and ψ be varied in consequence of the variation of m,

$$\cos(\theta + \psi) \times \delta\psi = \sin \theta \times \delta m.$$

Now when ψ is small, but in no other case, $\sin(\theta + \psi)$ is sensibly equal to $\sin \theta + \cos \theta \times \psi$; and in the second equation, the factor of the small quantity $\delta\psi$ may, without sensible error, be considered as $\cos \theta$. Thus, when ψ is small (whatever be the cause of that smallness), the following equations are sensibly accurate:

$$\sin \theta + \cos \theta \times \psi = \sin \theta + m \sin \theta, \quad \text{or } \cos \theta \times \psi = m \sin \theta;$$
$$\cos \theta \times \delta\psi = \sin \theta \times \delta m.$$

From which $\frac{\delta\psi}{\psi} = \frac{\delta m}{m}$; or, the proportion of the change of deviation (accompanying a variation in the colour of the light) to the whole deviation, is the same as the proportion of the change of the index of deviation to the whole index of deviation.

The two deviations which require consideration in the following investigation, namely, that produced by the atmosphere and that produced by a flint prism of small angle, are both small; the first because, though the angle of incidence is large, the index of deviation is very small; the second because, though the index of deviation is large, the angle of incidence is small. The theorem just found will, therefore, apply to both.

In collecting the numerical data to be employed for my calculation I have been referred by my friend Professor W. H. Miller to an account of Ketteler's observations on atmospheric refraction, given in the *Fortschritte der Physik*, 22nd Jahrgang, page 179. For the other media I use some well-known determinations by Fraunhofer. Although, in the practical construction, I use only flint glass, I have thought it interesting to insert the numbers for crown glass and water. The two rays of the spectrum by which I measure the dispersion are B and G.

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Medium.	Index of Deviation for B.	Index of deviation for G.
Air ...	0·00029350	0·00029873
Water ...	0·0330977	0·0341261
Crown 13 ...	0·0524312	0·0539908
Flint 30 ...	0·0623570	0·0655406

Now dividing the difference of the indexes of deviation for B and G by half their sum, for each medium, we obtain the following values of $\frac{\delta m \text{ (B to G)}}{m}$:—

Air	...	0·017662,
Water	...	0·030596,
Crown 13	...	0·029310,
Flint 30	...	0·049784.

Air, therefore, is much less dispersive in proportion to its refraction than either of the other media.

I now proceed to apply these numbers to the estimation of the effect of atmospheric dispersion on telescopic vision, and to the construction for its correction.

Let R be the atmospheric refraction of the object viewed; p the power of the telescope. Then the atmospheric dispersion (B to G), as seen by the naked eye, is $R \times 0{\cdot}01766$; and, as seen in the telescope, it is $R \times p \times 0{\cdot}01766$. A prism of flint glass is to be introduced close to the eye, whose dispersion is to neutralize this magnified atmospheric dispersion. Therefore the dispersion of the flint prism (B to G) must be $R \times p \times 0{\cdot}01766$; its refraction must be $R \times p \times 0{\cdot}01766 \times \frac{1}{0{\cdot}04978}$; and, since the index of deviation of the flint glass for the mean rays is 0·6395 (estimated as for the passage from glass to air, which, it is easily seen, give the proportion of the aggregate of deviations at the two surfaces to the refracting-angle of the prism), the refracting angle of the prism must be $\frac{R \times p \times 0{\cdot}01766}{0{\cdot}04978 \times 0{\cdot}6395}$. Performing the numerical calculation, we find that the angle of the flint prism must be $\frac{R \times p}{1{\cdot}8}$, or the atmospheric refraction must be

$$\frac{1{\cdot}8 \times \text{angle of the flint prism}}{p},$$

in order to produce perfect correction.

Suppose, then, that we have a series of flint prisms ground to the angles 2°, 4°, 6°, 8°, 12°, 16°. And suppose that we use a telescope with power 120 or with power 240. Then the following table, showing the zenith distance at which the atmospheric dispersion is corrected, is easily computed; the refraction being calculated by the formula just given, and the zenith-distance corresponding to the refraction being taken from a common table of refractions:—

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Angle of Flint Prism.	Telescopic power 120. Atmospheric Refraction.	Zenith Distance.	Telescopic power 240. Atmospheric Refraction.	Zenith Distance.
°	′	° ′	′	° ′
2	1·8	61 58	0·9	43 7
4	3·6	75 16	1·8	61 58
6	5·4	80 15	2·7	70 32
8	7·2	82 52	3·6	75 16
12	10·8	85 34	5·4	80 15
16	14·4	87 3	7·2	82 52

For view with the naked eye it would be necessary to use a prism (of appropriate small angle) with its edge downwards; but, for view with an inverting telescope, the edge of its appropriate prism must be upwards.

Mr. Simms has prepared, under my instruction, a series of prisms of flint glass with the angles just mentioned; and Mr. Carpenter has made trial of them. The following are two of Mr. Carpenter's reports :—

1869, July 15, $11^h\ 50^m$ to $12^h\ 20^m$. *Saturn*, zenith-distance about 82°, viewed with the Great Equatoreal, power 245. Atmospheric dispersion very bad.

Prism 12° corrected the dispersion perfectly.

Prism 8° corrected it fairly.

Prism 6° corrected it partially.

Prism 4° and 2° produced no visible effect.

Prism 16° confused the image.

1869, July 21, 9^h. The Moon, zenith-distance about 78°, viewed with the Altazimuth, power about 100.

Prism 2° slightly diminished the colour.

Prism 4° better than the last.

Prism 6° destroyed the colour entirely. The definition of the details about the Moon's rugged edge was much improved.

Prisms 8°, 12°, 16°, gave an opposite dispersion.

It will be seen here that the object which I proposed to myself is completely attained. It is made possible, by this construction, to examine a celestial body with delicacy and accuracy, under circumstances which would, without this construction, have rendered nice observation impossible.

The series of angles of the prism which I have given appears to me well adapted to general wants. I propose to furnish each of the principal telescopes to be used for the Transit of *Venus* with a complete series of such prisms, arranged perhaps on a long slider. Care must be taken to make the thickness of the slider-frame as small as possible, inasmuch as it must be accompanied with another slider carrying dark glasses. It will probably be

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found best to place both sliders between the two glasses of the eye-piece. This slightly disturbs the elements of the calculation above; but in practice the selection of the best prism will always be matter of trial, and the disturbance of calculations will be unimportant.

No assistance is given by this principle to the accurate determination of zenith-distances. The construction, which destroys the prismatic extension of a star's image, necessarily introduces an equal prismatic extension in the opposite direction on the breadth of the wire.

Before closing this subject I will advert to a remark made by one of the most acute telescope-observers who have ever been known in this Society, the late Rev. W. R. Dawes. He states that, in general, a telescope performs better with one particular point of the edge of its object-glass upwards than in any other position. The explanation of this singular remark will be found, I think, in the combination, of the effect of error of centering of the two lenses of an achromatic object-glass, with the effect of atmospheric dispersion. The centre of one lens (using the word "centre" to denote that part in which the tangent-planes of the two surfaces are parallel) ought to be exactly above the centre of the other lens. But it is not easy to make this adjustment perfect; the centre of one lens is frequently above a part of the other lens where the two surfaces have a slight inclination; and the refraction thus created produces in the image of every star a spectrum which rotates as the telescope-tube is made to rotate. In one position of the tube the atmospheric dispersion is opposed to this, and may wholly or in a great measure correct it; in the opposite position the atmospheric dispersion is added to it, and increases its injurious effects.

The atmospheric dispersion between B and G is about $\frac{1}{60}$th of the atmospheric refraction. At zenith-distance 45° it is nearly 1″, at 63° it is nearly 2″, at 80° about 5″. These are the lengths of the visible spectrum.

Royal Observatory, Greenwich,
1869, *August* 7.

Distribution of the Nebulæ. By R. A. Proctor, B.A.

Rather more than two years ago a paper by Mr. Cleveland Abbe on the distribution of the Nebulæ in space was read before this Society. That paper included a valuable contribution to science, in the form of a tabular statement exhibiting the manner in which the irresolvable Nebulæ are distributed over the celestial sphere. A few weeks before I had sent to the editor of the *Student* a paper on the same subject. Until this paper and four others dealing with the distribution of matter throughout that

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portion of space which falls within our cognisance had appeaerd, I did not feel at liberty to bring before this Society certain views which have been for a long time in my thoughts. Owing to the press of other matter, and especially of other astronomical matter in the *Student*, the series of papers was only completed last April. This is, therefore, the earliest opportunity I have had of submitting the views I speak of to the judgment of the Royal Astronomical Society.

The four pairs of maps now laid before the Society have been formed from the valuable table prepared by Mr. Abbe; but it is worthy of notice that their teachings differ in no important respect from those conveyed by maps formed from a similar table prepared by Sir John Herschel many years ago. It is important that this should be attended to; because (1) Herschel's table was formed from a catalogue of only 3812 nebulæ, while Mr. Abbe's was found from a catalogue of 5079 nebulæ; (2) Herschel's tabulation corresponded to a division of the sphere into spaces of 1^h in R.A. by 15° in Decl., while Mr. Abbe's took spaces of only 30^m in R.A. and 10° in Decl.; and (3) Herschel included all nebular objects (if I remember rightly) in his tabulation, while Mr. Abbe drew a distinction between the various orders of nebulæ, so that his table as finally drawn up included only 4053 irresolvable nebulæ. When we find that all the refinements thus introduced by Mr. Abbe, and an increase of nearly one-third in the total number of objects dealt with, produce no appreciable change in the indications which the nebulæ present of being aggregated together according to certain definite, and, as I think, very significant laws, we seem free to conclude that no extension of telescopic observation can appreciably affect our views respecting the distribution of the nebulæ. If the second tabulation had indicated laws appreciably different from those which the first had exhibited, the conclusion would have been that we must wait for an increase of knowledge before venturing to theorise; as it is, we may proceed to deal with the facts before us with some confidence as to the general accuracy of their indications. We see, then, that the two tables have an increased value when considered together.

I would first call attention to the pair of maps numbered I. The plan of projection is the isographic projection described in my *Handbook of the Stars.** The meridians and parallels are so drawn as to correspond to Mr. Abbe's tabulation, and in each space as many nebulæ as his table indicates are marked in, so as to spread pretty uniformly over the space. Thus the projection being isographic, the maps represent the actual distribution of the nebulæ (as to density of aggregation, &c.) over the northern and southern hemispheres.

Now when this is done we see that there is much more in the

* After that book was published I found that Sir J. Herschel had already suggested this construction. The coincidence is not very surprising when it is remembered that the problem of a central isographic projection is a very simple one, and has only one solution.

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arrangement of the nebulæ than that thinning off in the neighbourhood of the Milky Way which Mr. Abbe had recognised. No one can look at the general aspect of the two maps without seeing that the suggested distribution of the nebulæ in a figure roughly resembling a prolate spheroid, having the plane of the Milky Way at right angles to its longer axis, is wholly insufficient to account for the actual aspect of the maps. The northern map accords better with this view than the southern; but even in this map there is an irregularity in the clustering, an occasional evidence of streaminess, and in places a sharpness in the outline of the group which is altogether different from what would be seen if the nebulæ formed a figure even roughly resembling that suggested by Mr. Abbe. In the southern map all these features are exaggerated. In this map also a new and unexpected feature strikes us. We see that the Nubeculæ are associated by well-marked nebular streams with the main southern group.

But now it is obvious that the pair of maps numbered I. requires to be supplemented by maps which shall exhibit—(1) the two groups of nebulæ in their entirety, and (2) the great zone bare of nebulæ which divides one group from the other.

The pair of maps numbered II. is intended to satisfy condition (1) as nearly as can be done on equatorial maps. The projection corresponds to that used in maps I.* The first map of the pair exhibits the nebular group to the north of the Milky Way, the second exhibits the nebulæ to the south of that zone. It will be noticed that the contrast between the two groups is very striking.

The northern group presents a somewhat stratified aspect, and we can understand how it was that Sir W. Herschel, observing only in northern latitudes, conceived that the nebulæ might be found to be arranged in a zone somewhat resembling the Milky Way, but nearly at right angles to it. On a closer inspection, however, we recognise in the northern cluster a decidedly streamy character.†

* So far as I am aware the pairs of maps numbered II. and III. are the first in which the relations of the nebulæ have been presented isographically on any but a polar projection.

† It is to be noticed that it would be perfectly fair to strengthen the indications of these maps, by spreading the dots which represent the nebulæ less regularly over the spaces they belong to. At first sight this seems illegitimate. It might be argued, for example, that to take the streams presented to us on a first inspection, and to add to their obviousness by a special arrangement of the dots in the figure, is nothing less than to adopt an hypothesis, and to arrange our facts so as to seem to bear it out. But in reality this is not so. The process of arrangement so as to intensify the aggregations of dots and to make the vacuities more clearly apparent corresponds exactly to the process by which, having a series of ordinates in any graphic construction, we take a *curve* through their extremities instead of a series of rectangular cross-lines. The curved lines, in this case, slope upwards towards the direction of the longer ordinates, and downwards towards the direction of the shorter; and precisely in the same way the dots in any of the spaces in such maps as we are now dealing with should aggregate towards the direction in which there are greater aggregations, and be more

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The southern group of nebulæ presents features of a very interesting character. As in the northern group, the general aspect of the system is streamy, but this character is much more marked in the southern group. A peculiarity is also noticeable in the fact that the most striking aggregations are at the *extremities* of the streams. In fact, the central part of the whole group, a region exactly opposite the great northern aggregation of nebulæ, is almost vacant. We have, again, distinct evidence of the connexion between the Magellanic Clouds and the southern nebular group. The Nubeculæ are in fact merely instances, more marked than the others, of the tendency to aggregation at the extremities of the nebular streams which form the great southern group.

I now turn to the pair of maps numbered III., which serve to bring out more especially the great vacant zone. This I hold to be the most significant of all the features presented by the nebular system. It will be observed that there is no mistaking the fact that a true zone of nebular dispersion (if I may use that term as the converse of aggregation) exists in the heavens. Nor can the exact agreement of this zone with the galactic zone be called in question. I would now ask whether the conclusions to be drawn from these peculiarities ought not to be somewhat different from those which have been commonly accepted.

In the first place, I would remark that Sir W. Herschel was disposed to look on the position of the zone of nebulæ, which he supposed to exist in the heavens, at right angles, or nearly so, to the Milky Way, as not accidental. The peculiarity we actually have to deal with, however, is much more significant; first, because the agreement of two zones in position is a circumstance which is antecedently much more improbable than mere perpendicularity of intersection; and secondly, because the existence of a zone of dispersion is in itself a much more remarkable circumstance than the existence of a zone of aggregation. On the first point, it is merely necessary to remark that to secure perpendicularity between two zones it is requisite only that the poles of one zone should lie upon the other, which can happen in an infinite number of ways; whereas, to secure coincidence, the poles of one zone must coincide in position with the poles of the other, which can happen only in one way. On the second point, it may be noticed that the existence of a zone of dispersion would be very remarkable even if the zone were not particularly well marked;

sparsely strewn in the direction towards which there are vacuities. Had this been done the streaminess already apparent in the arrangement of the nebulæ would have been rendered much more obvious. In the large diagram* I have felt justified in following this plan. Indeed so far from being unfair, it may be shown to be that which, according to the doctrine of chances, gives the arrangement which would most probably be observed if each nebula had been marked in its proper place.

* This diagram was exhibited at the Meeting.

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because all that we know of the universe leads us to look upon systems aggregated into the figures of cylinders, prolate spheroids, or the like, as much less likely to be met with than systems in the figure of disks, rings, oblate spheroids, or the like; and it is only figures of the former sort which can result in showing a zone of dispersion, figures of the latter kind exhibiting (as in the case of the sidereal system) a zone of aggregation. But a very well-marked zone of dispersion, as in the case of the nebular system, is a yet more significant phenomenon, because it can only be explained by looking upon the aggregations between which it lies as distinct systems. Looking on the two nebular clusters in this light, the fact that the Milky Way agrees exactly in position with the zone which separates them, becomes one which we are bound to account for in forming any theory of the nebulæ. In fact, when dealing with the aggregation of nebulæ near the north pole of the galactic system, Sir John Herschel expressed this very opinion; though he has nowhere indicated, so far as I am aware, the conclusion which seems to me to follow directly from that opinion:—

I cannot see how we can look upon the coincidence I have spoken of as *not* accidental, without being led to the conclusion that the nebular and stellar systems are parts of a single scheme. If the nebulæ were external star-galaxies, the coincidence might have happened, but it would be accidental. If they formed a system "distinct from the sidereal system, though involving, and perhaps to a certain extent intermixed with the latter," as Sir John Herschel suggested, the coincidence would still be accidental. If "our system" (again to quote Sir J. Herschel) "lies outside the denser part of the cluster, but is involved within one of its outlying members," or "forms *an element* of some one of its protuberances or branches," yet, again, the coincidence would be accidental. No theory, which looks upon our galaxy as simply a member of the nebular system can possibly be reconciled with the view that the position of the zone of nebular dispersion is otherwise than accidental, unless we assume either (i.) that *all* the members of the system, or at least all in our neighbourhood, have a position parallel to that of our galaxy, or at right angles to the length of the nebular system, or (ii.) that there is some peculiarity in the galactic stratum preventing us from looking so far out into space along its length than elsewhere. The first supposition is disposed of by the briefest study of the nebulæ, which (looked upon as disk-shaped universes) have every variety of position. The second has a greater appearance of probability, yet it would be easy to dispose of it on its own merits. It must be dismissed, however, for a reason associated with a matter which yet remains to be considered.

We have seen that the nebulæ belonging to the Magellanic clouds are associated in a very obvious manner with the rest of the nebular system. But it must not be forgotten that these strange objects are also intimately associated with the sidereal

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system. In fact, their visibility to the naked eye is due entirely to the fact that they contain a number of small stars. Here, then, the sidereal and nebular systems are seen as they are nowhere else to be observed, *intermixed.* Perversely enough, this peculiarity, which seems so obviously to indicate an intimate association between the two systems, has been held by many distinguished astronomers, to prove that the Nubeculæ belong neither to the sidereal nor to the nebular system. Sir John Herschel has distinctly pointed out the conclusions which should be drawn from the evidence given by the Nubeculæ; yet he nowhere definitely rejects views to which those conclusions seem directly opposed. He simply remarks that we may be led to look with some suspicion upon views which "in former pages had been somewhat positively insisted upon."*

The evidence afforded by the Nubeculæ seems to me decisive in favour of the intimate association between the stellar and nebular systems, which I have been endeavouring to establish. When it is combined with the evidence before adduced, no reasonable doubt can exist, I think, that the stars and nebulæ are members of the same scheme. There may be individual nebulæ which are true external universes,—It is possible, for example, that the spiral nebulæ may be of this class, as also perhaps the *Andromeda* nebula, and a few others,—but I consider that for one nebula which is really external there are hundreds which are associated with the sidereal system.

What the processes may be which have led to the present arrangement of the great scheme of stars and nebulæ, I shall not venture even to conjecture. I may remark, however, that the great majority of the nebulæ constituting the northern and southern clusters may be looked upon as owing their present constitution to the fact that they are outside the region of most active stellar aggregation, and not to any difference in their original structure. I believe that the irresolvable and scarcely resolvable nebulæ are composed of stars really smaller and really closer than those forming the clusters and easily resolvable nebulæ. In favour of this view I would adduce a peculiarity exhibited in Mr. Cleveland Abbe's tabulation of the nebulæ, the true meaning of which seems to have escaped him.

* I may remark here, in passing, that the tentative and philosophical tone in which Sir John Herschel has reasoned respecting the universe, the light grasp with which he holds the theories he has himself developed, and the wide range over which his views extend have, on the one hand, encouraged me to adopt views opposed to those which he has sanctioned by his approval, and, on the other, to put forward views which he has already mentioned, but to which I have been led by independent considerations. His whole treatment of the subject of the sidereal and nebular systems indicates that the views he has put forward are intended for the most part rather as suggestions than as theories. This power of weighing evidence without reference to preconceived opinions; of indicating the theories suggested by this or that portion of the evidence, and as readily indicating reasons which must lead to the abandonment of such theories, may be looked upon as characteristic of the Herschel family. It would be well for the cause of scientific progress if the power were not so uncommon as it is.

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If the objects in Sir J. Herschel's Catalogue are arranged in the following order,—(i.), clusters; (ii.), easily resolvable globular clusters; (iii.), resolvable globular clusters; (iv.), resolvable nebulæ; and (v.), irresolvable nebulæ, and the arrangement of these classes of objects with reference to a galactic zone 10° wide be considered, we find that objects in class (i.) are obviously aggregated on the galactic zone; objects in class (ii.) less so; those in class (iii.) still less so; those in class (iv.) are aggregated away from the galactic zone; and those in class (v.) are aggregated almost exclusively outside that zone. When we take a galactic zone 30° wide, we have,—objects in class (i.) almost exclusively on the galactic zone; those in class (ii.) decidedly aggregated there; those in class (iii.) somewhat less so; while those in the other classes present much the same relations as before.

This plainly shows that the first three of these classes are associated with the galaxy in proportion to their resolvability; the fourth class, which is still resolvable, is removed from the neighbourhood of the galaxy; and the fifth still more so. Now, can we look upon the distinction between the resolvable nebulæ and the resolvable clusters as so marked that, whereas we can admit the latter to be associated with the galaxy (or rather we are forced to do so), we must look upon the former as lying far away beyond the range of the sidereal system? But even if we must do so, how can we then account for the fact that the irresolvable nebulæ show a greater antipathy to the neighbourhood of the galaxy than their resolvable fellow-systems? If we do not, if we look on the resolvable nebulæ as belonging to the sidereal scheme, notwithstanding their aggregation away from the galactic zone, what reason can we assign for forming a different opinion of the irresolvable nebulæ which exhibit precisely the same relation, only in a more marked form? Finally, if we admit the irresolvable nebulæ into the galactic scheme, we cannot look upon greater distance as accounting for their irresolvability, because once they are recognised as associated with the galaxy, we can see no reason for the more distant members of the nebular part of the universe lying towards the poles of the galactic zone. The antecedent probability of such a relation prevailing, that *distance* is the true cause of the irresolvability of the objects forming the great nebular clusters, is exceedingly minute. Of course, *cæteris paribus*, the more distant a cluster is, the less easily resolvable it will be. But we must look for some other cause than distance to account for the fact that nebulæ seen outside the milky way are less easily resolvable than nebulæ seen upon that zone. I believe that cause may be assumed not unreasonably to be the difference in the circumstances under which the galactic and the extra-galactic nebulæ have reached their present state.

I have hitherto omitted all reference to the gaseous nebulæ. So far as present observation goes, these objects show a decided preference for the neighbourhood of the galactic zone. The planetary nebulæ, all of which are probably gaseous, are so distri-

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buted, that two-thirds are on the wider galactic zone referred to above, whose area is less than one-fourth that of the whole sphere. The irregular gaseous nebulæ are all on or close to the Milky Way, except one, which is in the greater Magellanic Cloud; another proof, if any were wanting, that the Nubeculæ are not external systems.

In the pair of maps numbered IV. I have combined the visible sidereal system with the nebular scheme; on the same isographic projection, as in maps I. My object was to discover, if possible, whether any laws can be discovered associating the nebular and sidereal systems together. I notice that, outside the Milky Way, the nebulæ seem to be associated with the lucid stars in this way, that, where there are many leading brilliants, there nebulæ seem densest. It is to be noticed that there is not a single marked vacancy in either of the nebular groups where there is not at the same time a marked absence of bright stars. I would point out, also, that, of the two most marked nebular streams, one is coincident with the stellar stream recognised by the ancients in their figuring of the constellation *Eridanus* (the prolongation southwards being recognised by modern astronomers in the constellations *Hydrus* and *Reticulum*); the other coincides with the stellar stream associated by the ancients with the water from the vase of *Aquarius*. This association between stellar and nebular aggregation seems to occupy a character midway between the yet closer relation observed in the Nubeculæ, and the directly contrary relation observed in the neighbourhood of the Milky Way.

On a Method of Determining the Dimensions of the Disks of those Stars which are liable to be Occulted by the Moon. By Richard A. Proctor, B.A.

The following plan for measuring star-disks is theoretically sound, but may probably present great difficulties in practice. It seems worth being tried, however.

If the image of a star is put in rapid rotation (which may be done in many ways) while the centre of rotation moves in a circle, in a period exceeding the maximum duration of luminous impressions on the retina, the appearance presented to the eye will be that of an epicycloidal coil of light, whose rapidly advancing end is always on a black ground. Now if the eye be intently fixed on this coil just before the star's occultation by the Moon, it is obvious that at the instant of occultation the whole coil will vanish; but, during the brief interval which the luminous impression on the retina occupies in vanishing, there will be time to notice the appearance of that end of the coil which was last formed. Now if we suppose the rapidity of rotation such that a quarter of a revolution was made while the Moon was traversing

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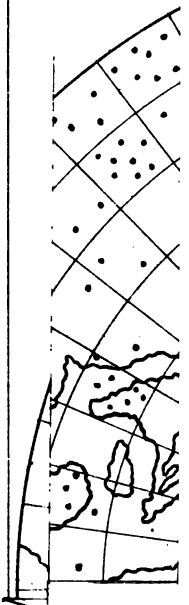

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the star's disk, it is clear that along that quadrant there will be a gradual diminution of light down to evanescence at the extremity. If the star had an apparent diameter only half as large, the arc of fading light would be but half a quadrant, and so on. The observation would be rendered easier by the fact that a neighbouring part of the coil would always give the means of a ready comparison. I find that if the star's image rotated 100 times in a second, a star ten times as far off as *α Centauri*, and having a real diameter as large as our Sun's, would give an arc of fading light about $55\frac{1}{4}°$ in length. Here I suppose the occultation direct. An oblique occultation would obviously be more favourable.

The light of the star would be much reduced by so rapid a rotation; but with a large telescope I believe quite enough light would be obtained even from a star of the lower magnitudes for the purpose in view. Up to a certain point the reduction of light would be advantageous, as it is much easier to detect differences between faint than between brilliant lights.

An observer would require special training for the use of this method, and experimental observations on terrestrial lights would probably be required before any great reliance could be placed on the resulting measurements of stellar disks.

If reliable measures could be obtained however, the results would obviously be most important. We should learn what relations subsist between the real and the apparent magnitudes of stars, and thus what varieties there may be in the intrinsic brilliancies of these orbs; we should have a ready means of detecting the close duplicity of many apparently single stars; and we could not fail to obtain valuable information on other points of interest. One of the chief ends which the successful application of this method would subserve would be to afford us information as to the possible extinction of light in traversing the inter-sidereal spaces.

As respects this last point I would suggest that a careful comparison of the spectra of faint stars with those of the brighter orbs might indicate the occurrence of extinction; since there is reason for anticipating that if extinction really takes place in space there would be signs of *elective* absorption. If certain portions of the spectra of the smaller stars were found to be relatively fainter than the corresponding portions of the spectra of the brighter stars, or if a tendency to such a relation were noted on the average (for we must not forget that many small stars may be near to us), we should have a strong argument in favour of the theory that light is extinguished in the course of its progress through space.

Sept. 11th, 1869.

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